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ABSTRACT

This paper estimates the effect of education on participation in crime and incarceration, using data from the U.S. Census and changes in state compulsory attendance laws. Increases in compulsory schooling ages are not correlated with increases in state resources devoted to fighting crime. Research suggests that additional years of secondary schooling reduce the probability of incarceration, with the greatest impact associated with completing high school. Black-white differences in educational attainment explain up to 23 percent of the gap in incarceration rates. FBI arrest data corroborate findings on incarceration. The main impacts of education relate to murder, assault, and motor vehicle theft. The study examines the effect of schooling on self-reported crime in the National Longitudinal Survey of Youth, noting that estimates for imprisonment and arrest are caused by changes in criminal behavior, not educational differences in the probability of arrest or incarceration conditional on crime. The study calculates social savings from crime reduction associated with high school graduation. The externality is about 14-26 percent of the private return, suggesting that a significant part of the social return to completing high school comes in the form of externalities from crime reduction. (Contains 12 tables and 37 references.) (SM)

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The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports*

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Abstract

This paper estimates the effect of education on participation in criminal activity accounting for endogeneity of schooling. Crime is a negative externality with enormous social costs, so if education reduces crime, then schooling may have large social benefits that are not taken into account by individuals.

The paper begins by analyzing the effect of schooling on incarceration using Census data and changes in state compulsory attendance laws over time as an instrument for schooling. Changes in these laws have a significant effect on educational achievement, and we reject tests for reverse causality. Moreover, increases in compulsory schooling ages are not correlated with increases in state resources devoted to fighting crime. Both OLS and IV estimates agree and suggest that additional years of secondary schooling reduce the probability of incarceration with the greatest impact associated with completing high school. Differences in educational attainment between blacks and whites can explain as much as 23% of the black-white gap in incarceration rates.

We corroborate our findings on incarceration using FBI data on arrests that distinguish among different types of crimes. The biggest impacts of education are associated with murder, assault, and motor vehicle theft. We also examine the effect of schooling on self-reported crime in the NLSY and find that our estimates for imprisonment and arrest are caused by changes in criminal behavior and not educational differences in the probability of arrest or incarceration conditional on crime. Given the consistency of our estimates, we calculate the social savings from crime reduction associated with high school graduation. The externality is about 14-26% of the private return, suggesting that a significant part of the social return to completing high school comes in the form of externalities from crime reduction.

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1 Introduction

Is it possible to reduce crime rates by raising the education of potential criminals? If so, would it be cost effective with respect to other crime prevention measures? Despite the enormous policy implications, little is known about the relationship between schooling and criminal behavior.

The motivation for these questions is not limited to the obvious policy implications for crime prevention. Estimating the effect of education on criminal activity may shed some light on the magnitude of the *social return* to education. Economists interested in the benefits of schooling have traditionally focused on the private return to education. However, researchers have recently started to investigate whether schooling generates benefits beyond the private returns received by individuals. In particular, a number of studies attempt to determine whether the schooling of one worker raises the earnings of other workers around him. (For example, see Acemoglu and Angrist (2000), Heckman and Klenow, (1999), and Moretti (1999).) Yet, little research has been undertaken to evaluate the importance of other types of external benefits of education, such as its potential effects on crime. (Lochner (1999) and Witte (1997) are notable exceptions.) Crime is a negative externality with enormous social costs. If education reduces crime, then schooling will have social benefits that are not taken into account by individuals. In this case, the *social return* to education may exceed the private return. Given the large social costs of crime, even small reductions in crime associated with education may be economically important.

There are a number of reasons to believe that education can reduce criminal activity. First, schooling increases the returns to legitimate work, raising the opportunity costs of illegal behavior.[1] Additionally, punishment for criminal behavior often entails incarceration. By raising wage rates, schooling makes any time spent out of the labor market more costly. Second, schooling may directly affect the financial or psychic rewards from crime itself. Finally, schooling may alter preferences in indirect ways, which may affect decisions to engage in crime. For example, education may increase one's patience (as in Becker and Mulligan (1997)) or risk aversion.

Despite the many reasons to expect a causal link between education and crime, empirical research is not conclusive.[2] The key difficulty in estimating the effect of education on criminal

[1] Freeman (1996), Gould, et al. (2000), Grogger (1998), Machin and Meghir (2000), and Viscusi (1986) empirically establish a negative correlation between earnings levels (or wage rates) and criminal activity. The relationship between crime and unemployment has been more tenuous (see Chiricos (1987) or Freeman (1983, 1995) for excellent surveys); however, a number of recent studies that better address problems with endogeneity and unobserved correlates (including Gould, et al. (2000) and Raphael and Winter-Ebmer (2001)) find a sizeable positive effect of unemployment on crime.

[2] Witte (1997) concludes that "...neither years of schooling completed nor receipt of a high school degree has a significant affect on an individual's level of criminal activity." But, this conclusion is based on only a few available studies, including Tauchen, et al. (1994) and Witte and Tauchen (1994), which find no significant link between education and crime after controlling for a number of individual characteristics. While Grogger (1998) estimates a significant negative relationship between wage rates and crime, he finds no relationship between education and crime after controlling for wages. (Of course, increased wages are an important consequence of schooling.) More recently, Lochner (1999) estimates a significant and important link between high school graduation and crime using data

activity is that unobserved characteristics affecting schooling decisions are likely to be correlated with unobservables influencing the decision to engage in crime. For example, individuals with high criminal returns or discount rates are likely to spend much of their time engaged in crime rather than work regardless of their educational background. To the extent that schooling does not raise criminal returns, there is little reward to finishing high school or attending college for these individuals. As a result, we might expect a negative correlation between crime and education even if there is no causal effect of education on crime. State policies may induce bias with the opposite sign — if increases in state spending for crime prevention and prison construction trade off with spending for public education, a *positive* spurious correlation between education and crime is also possible.

In this paper, we use individual-level data on incarceration from the Census and cohort-level data on arrests by state from the FBI Uniform Crime Reports (UCR) to analyze the effects of schooling on crime. We then turn to self-report data on criminal activity from the National Longitudinal Survey of Youth (NLSY) to verify that the estimated impacts measure changes in crime and not educational differences in the probability of arrest or incarceration conditional on crime. We employ a number of empirical strategies to account for unobservable individual characteristics and state policies that may introduce spurious correlation.

We start by analyzing the effect of education on incarceration. The group quarters type of residence in the Census indicates whether an individual is incarcerated at the Census date. For both blacks and whites, OLS estimates uncover significant reductions in the probability of incarceration associated with more schooling. To address endogeneity problems, we use changes in state compulsory attendance laws over time to instrument for schooling. Changes in these laws have a significant effect on educational achievement, and we reject tests for reverse causality. In the years preceding increases in compulsory schooling laws, there is no obvious trend in schooling achievement. Increases in education associated with increased compulsory schooling take place *after* changes in the law. Furthermore, increases in the number of years of compulsory attendance raise high school graduation rates but have no effect on college graduation rates. These two facts indicate that the increases in compulsory schooling raise education, not vice versa. We also examine whether increases in compulsory schooling ages are associated with increases in state resources devoted to fighting crime. They are not.

Instrumental variable estimates reveal a significant relationship between education and incarceration, and they suggest that the impacts are greater for blacks than for whites. One extra

from the National Longitudinal Survey of Youth (NLSY). Other research relevant to the link between education and crime has examined the correlation between crime and time spent in school (Gottfredson 1985, Farrington et al. 1986, Witte and Tauchen 1994). These studies find that time spent in school significantly reduces criminal activity — more so than time spent at work — suggesting a contemporaneous link between school attendance and crime. Previous empirical studies have not controlled for the endogeneity of schooling.

year of schooling results in a .10 percentage point reduction in the probability of incarceration for whites, and a .37 percentage point reduction for blacks. To help in interpreting the size of these impacts, we calculate how much of the black-white gap in incarceration rates in 1980 is due to differences in educational attainment. Differences in average education between blacks and whites can explain as much as 23% of the black-white gap in incarceration rates.

Because incarceration data do not distinguish between types of offenses, we also examine the impact of education on arrests using data from the UCR. This data allows us to identify the type of crime that arrested individuals have been charged with. Estimates uncover a robust and significant effect of high school graduation on arrests for both violent and property crimes, effects which are consistent with the magnitude of impacts observed for incarceration in the Census data. When arrests are separately analyzed by crime, the greatest impacts of graduation are associated with murder, assault, and motor vehicle theft.

Estimates using arrest and imprisonment measures of crime may confound the effect of education on criminal activity with educational differences in the probability of arrest and sentencing conditional on commission of a crime. To verify that our estimates identify a relationship between education and actual crime, we estimate the effects of schooling on self-reported criminal participation using data from the NLSY. These estimates confirm that education significantly reduces self-reported participation in both violent and property crime. We also use the NLSY to explore the robustness of our findings on imprisonment to the inclusion of rich measures of family background and individual ability. The OLS estimates obtained in the NLSY controlling for AFQT scores, parental education, family income, and several other background characteristics are remarkably similar to the estimates obtained using Census data.

Given the general consistency in findings across data sets, measures of criminal activity, and identification strategies, we cannot reject that a relationship between education and crime exists. Using our estimates, we calculate the social savings from crime reduction associated with high school completion. Our estimates suggest that a 1% increase in male high school graduation rates would save as much as $1.4 billion, or about $2,100 per additional male high school graduate. These social savings represent an important externality of education that has not yet been documented. The estimated externality from education ranges from 14-26% of the private return to high school graduation, suggesting that a significant part of the *social return* to education is in the form of externalities from crime reduction.

The remainder of the paper is organized as follows. In Section 2, a simple model of criminal activity is described. Section 3 reports estimates of the impact of schooling on incarceration rates (Census data), and Section 4 reports estimates of the impact of schooling on arrest rates (UCR data). Section 5 uses NLSY data on self-reported crime and on incarceration to check the robustness of UCR and Census-based estimates. In Section 6, we calculate the social savings from

crime reduction associated with high school graduation. Section 7 concludes.

2 An Economic Framework

To provide some intuition as to why education might affect criminal behavior, this section discusses a simple economic model of work, school, and crime. The model is by no means a complete description of criminal decisionmaking, but it is a useful reference point for an empirical study of crime and schooling. Individuals are assumed to choose the amount of education they acquire and the amount of time spent on work and crime once they have finished school. We begin by analyzing crime and work decisions conditional on educational choice, then return to the educational choice problem.

Consider the decisions of someone who has completed s years of school and must decide how to allocate his time to work and crime, where k_t is the fraction of time spent committing crime at age t. Let $w_t(s)$ represent his wage rate and $R(k_t, s)$ his total net return from crime at age t if he has s years of schooling.[3] Assume that someone who commits crime in period t has a probability, $\pi(k_t)$, of being punished the next period, $t+1$, where $\pi'(k_t) > 0$. For simplicity, assume that the punishment, P, is constant over time and measured in utility terms. While in school, an individual receives a constant utility of $\bar{u}$. Since we are interested in post-school crime, we ignore crime during school years. At each age after completing school, individuals consume their income from work and crime, receiving utility $u(y_t)$, where $y_t = w_t(s)(1-k_t) + R(k_t, s)$ is total income in period t, $u'(y_t) > 0$ and $u''(y_t) \leq 0$. The individual's maximization problem, conditional on already having chosen s years of school, is

$$V(s) = \max_{\{k_t\}_{t=s+1}^{T}} \left\{ \sum_{t=0}^{s-1} \beta^t \bar{u} + \beta^{s-1} \sum_{t=s+1}^{T} \rho^{t-s}(s)[u(w_t(s)(1-k_t) + R(k_t, s)) - \rho(s)\pi(k_t)P] \right\},$$

where $\beta \in [0,1]$ is an individual's initial discount factor, $\rho(s) \in [0,1]$ is his after-school discount factor (i.e. schooling may affect his rate of time preference), and T is the total number of years he can work or attend school. $V(s)$ represents the lifetime value of choosing s years of school when the individual chooses his criminal behavior optimally.

The interior first order condition for crime, k_t, is given by

$$\frac{\partial R(k_t, s)}{\partial k_t} - w_t(s) = \rho(s)\left(\frac{\pi'(k_t)P}{u'(y_t)}\right) \geq 0. \tag{1}$$

Notice, the gap between current returns from crime and work must be (weakly) positive, since crime involves future punishment. The right hand side of equation (1) represents the compensating differential that must be paid in the criminal sector due to potential punishment. It is

[3] In the analysis that follows, we assume that $w_t'(s) > 0$, $w_t'(s) > \frac{\partial^2 R}{\partial k_t \partial s}$, and $\frac{\partial R}{\partial s} \geq 0$.

increasing in the discount rate $\rho(s)$, since impatient individuals (low $\rho(s)$) heavily discount future punishment costs.

Equation (1) suggests three ways that schooling can affect criminal decisions. First, schooling increases individual wage rates, thereby increasing the opportunity costs of crime. Second, schooling may affect the net marginal returns to crime, $\frac{\partial R}{\partial k_t}$. Finally, schooling may alter individual rates of time preference. That is, schooling may increase the patience exhibited by individuals (i.e. $\rho'(s) > 0$).[4] As long as schooling increases the marginal return to work more than crime ($w_t'(s) > \frac{\partial^2 R}{\partial k_t \partial s}$) and schooling does not decrease patience levels, crime is decreasing in the number of years of schooling. It is also clear that, all else equal, individuals with higher wage rates and lower discount factors (ρ) will commit less crime. High wage rates reduce crime by increasing its opportunity cost, while increased patience makes delayed punishments more costly.

Recall that schooling is not exogenous. After calculating their optimal lifetime work and crime decisions for each potential level of schooling, young individuals will choose the education level that maximizes lifetime earnings, $V(s)$. The same factors that affect decisions to commit crime, and therefore $V(s)$, also affect schooling decisions. For example, it is clear from equation (1) that individuals with lower discount factors will engage in more crime, since more impatient individuals put less weight on future punishments. At the same time, individuals with low discount factors choose to invest less in schooling, since they discount the future benefits of schooling more heavily. Similarly, individuals with a high marginal return from crime are likely to spend much of their time committing crime regardless of their educational attainment. If schooling provides little or no return in the criminal sector (i.e. $\frac{\partial^2 R}{\partial k_t \partial s} \approx 0$), then there is little value to attending school. Both examples suggest that schooling and crime are likely to be negatively correlated, even if schooling has no causal effect on crime.

An Empirical Specification

We now clarify the empirical issues involved in estimation. To simplify the discussion further, consider an income maximizing framework ($u(y) = y$) where the probability of facing punishment is simply πk_t and the net return to crime depends on individual characteristics θ according to[5]

$$R(k_t, s) = (\theta + \tau(s))k_t - \frac{\eta}{2}k_t^2.$$

[4] While this specification does not explicitly allow schooling to affect individual tastes for crime, the relationship between schooling and net criminal returns yields qualitatively similar implications. One could also allow for incarceration (i.e. punishment may include time out of the criminal and labor market), which would make punishments more costly for those with high wage rates or criminal returns. This would provide another channel through which schooling affects crime. These additional channels have been suppressed to maintain simplicity.

[5] We assume throughout this analysis that θ is unobserved. Introducing observed characteristics is straightforward.

Solving the first order conditions for k_t yields the following characterization for criminal activity:[6] $k_t(\theta, s) = (1/\eta)(-w_t(s) + \gamma(s) + \theta - \rho(s)\pi P)$. If time preferences are unaffected by schooling and the probability of punishment as well as the punishment itself varies across locations, l, then criminal behavior for person i at age t is described by

$$k_{i,l,t} = -\frac{w_t(s_i) - \gamma(s_i)}{\eta} + \frac{\theta_i}{\eta} - \frac{\rho\pi_l P_l}{\eta}$$

If the difference in returns to schooling for work and crime is linear in schooling, so $(w_t(s) - \gamma(s))/\eta = -c_t + \delta s$, we obtain a simple reduced form specification for criminal behavior for person i as

$$k_{i,l,t} = c_t - \delta s_i + \tilde{\theta}_i - d_l$$

where δ is the parameter of interest, $\tilde{\theta}_i = \theta_i/\eta$ is a measure of unobserved ability, and $d_l = \rho\pi_l P_l/\eta$ is a state-specific dummy representing the deterrent effect of expected punishment. Standard OLS regressions of criminal activity on schooling and current location will be biased if individual criminal abilities or returns (θ_i) are correlated with schooling. Indeed, theory suggests that θ_i and s_i are negatively correlated (i.e. those with high criminal returns will acquire less education) producing a *negative* bias in the estimated impact of schooling on crime. Failure to account for differences in expected punishment levels across location may also induce a bias. If states face budget decisions between funding for schools and funding for police and prisons, we might expect a negative correlation between expected punishments and schooling levels across states. In this case, failure to account for state-level variation in expected punishments would lead to *positively* biased estimates.

To address the endogeneity of schooling and eliminate the bias induced by correlation between θ_i and s_i, we use instruments that exogenously affect schooling choices. Using valid instruments for schooling and controlling for state-level variation should produce unbiased estimates of δ in the simple framework above. A final complication arises due to data limitations. The instruments that we use in this paper are effective when using large data sets on crime like the Census or UCR. However, neither of these data sets measures crime directly. The Census data provide information on incarceration while the UCR provide data on arrests. It is, therefore, important to clarify the relationship between schooling and these alternative measures of crime.

It is reasonable to assume that arrests and incarceration are a function of the amount of crime committed, k_t. Consider first the case where both the probability of arrest conditional on crime (π_a) and the probability of incarceration conditional on arrest (π_i) are constant and age invariant. Then an individual with s years of schooling will be arrested with probability $Pr(Arrest_t) = \pi_a k_t(s)$ and incarcerated with probability $Pr(Inc_t) = \pi_i\pi_a k_t(s)$.

[6] For an interior solution where $\theta \in (\alpha_t(s), \alpha_t(s) + \eta]$ and $\alpha_t(s) = w_t(s) - \gamma(s) + \rho(s)\pi P$.

Consider two schooling levels – high school completion (s=1) and drop out (s=0). Then, the effect of graduation on crime is simply $k_t(1) - k_t(0)$, while its effect on arrests is $\pi_a(k_t(1) - k_t(0))$. Its impact on incarceration is $\pi_i\pi_a(k_t(1) - k_t(0))$. The measured effects of graduation on arrest and incarceration rates are less than its effect on crime by factors of π_a and $\pi_i\pi_a$, respectively. However, graduation should have similar effects on crime, arrests, and incarceration when measured in logarithms.

More generally, the probability of arrest conditional on crime, $\pi_a(s)$, and the probability of incarceration conditional on arrest, $\pi_i(s)$, may depend on schooling. This would be the case if, for example, more educated individuals have access to better legal defense resources or are treated more leniently by police officers and judges. In this case, the measured effects of graduation on arrest and incarceration rates (when measured in logarithms) are

$$\ln Pr(Arrest_t|s=1) - \ln Pr(Arrest_t|s=0) = (\ln k_t(1) - \ln k_t(0)) + (\ln \pi_a(1) - \ln \pi_a(0))$$

and

$$\ln Pr(Inc_t|s=1) - \ln Pr(Inc_t|s=0) = (\ln k_t(1) - \ln k_t(0)) + (\ln \pi_a(1) - \ln \pi_a(0)) + (\ln \pi_i(1) - \ln \pi_i(0)),$$

respectively. If the probability of arrest conditional on crime and the probability of incarceration conditional on arrest are larger for less educated individuals, then the measured effect of graduation on arrest is greater than its effect on crime by $\ln \pi_a(1) - \ln \pi_a(0)$ and its measured effect on imprisonment is larger still by the additional amount $\ln \pi_i(1) - \ln \pi_i(0)$. Mustard (2001) provides evidence from U.S. federal court sentencing that high school graduates are likely to receive a slightly shorter sentence than otherwise similar graduates, though the difference is quite small (about 2-3%).

3 The Impact of Schooling on Incarceration Rates

3.1 Data and OLS Estimates

We begin by analyzing the impact of education on the probability of incarceration for men using U.S. Census data. The public versions of the 1960, 1970, and 1980 Censuses report the type of group quarters and, therefore, allow us to identify prison and jail inmates, who respond to the same Census questionnaire as the general population. We create a dummy variable equal to 1 if the respondent is in a correctional institution.[7] We include in our sample males ages 20-60 for whom all the relevant variables are reported. Summary statistics are provided in Table 1. Roughly 0.5-0.7% of the respondents are in prison during each of the Census years we examine. Average years of schooling increase steadily from 10.5 in 1960 to 12.5 in 1980.

[7] Unfortunately, the public version of the 1990 Census does not identify inmates. The years under consideration precede the massive prison build-up that began around 1980.

Table 2 reports incarceration rates by race and educational attainment. The probability of imprisonment is substantially larger for blacks than for whites and this is the case for all years and education categories. Incarceration rates for white men with less than twelve years of schooling are around .8% while they average about 3.6% for blacks over the three decades. Incarceration rates are monotonically declining with education for all years and for both blacks and whites.

An important feature to notice in Table 2 is that the reduction in the probability of imprisonment associated with higher schooling is substantially larger for blacks than for whites. For example, in 1980 the difference between high school drop outs and college graduates is .8% for whites and 3.5% for blacks. Because high school drop outs are likely to differ in many respects from individuals with more education, these differences do not necessarily represent the causal effect of education on the probability of incarceration. However, the patterns indicate that the effect may differ for blacks and whites. In the empirical analysis below, we allow for differential effects by race whenever possible.[8]

To account for other factors in determining incarceration rates, we begin by using OLS to examine the impacts of education. Figure 1 shows how education affects the probability of imprisonment at all schooling levels after controlling for age, state of birth, state of residence, cohort of birth and year effects (i.e. the graphs display the coefficient estimates on the complete set of schooling dummies). The figure clearly shows a decline in incarceration rates with schooling beyond 8th grade, with a larger decline at the high school graduation stage than at any other schooling progression.

We now present results for models that are either linear in years of schooling or that measure education using a dummy variable for high school graduation. Table 3 reports the estimated effects of years of schooling on the probability of incarceration using a linear probability model (columns 1-3). Estimates for whites are presented in the top panel with estimates for blacks in the bottom. In column 1, covariates include year dummies, age (14 dummies for three-year age groups, including 20-22, 23-25, 26-28, etc.), state of birth, and state of current residence, which are all likely to be important determinants of criminal behavior and incarceration.[9] To account for the many changes that affected Southern born blacks after Brown v. Board of Education, we also include a state of birth specific dummy for black men born in the South who turn age 14 in 1958 or later. These estimates suggest that an additional year of schooling reduces the probability of incarceration by 0.001 for whites and by 0.0037 for blacks.[10] The larger effect for blacks is

[8] The stability in aggregate incarceration rates reported in Table 1 masks the underlying trends within each education group, which show substantial increases over the 1970s. The substantial difference in high school graduate and drop out incarceration rates combined with the more than 25% increase in high school graduation rates over this time period explains why aggregate incarceration rates remained relatively stable over time while within education group incarceration rates rose.

[9] All specifications exclude Alaska and Hawaii as a place of birth, since our instruments below are unavailable for those states.

[10] The standard errors are corrected for state of birth - year of birth clustering, since our instrument below varies

consistent with the larger differences in unconditional means reported in Table 2.

Column 2 accounts for unobserved differences across birth cohorts, allowing for differences in school quality or youth environments by including dummies for decade of birth (1914-1923, 1924-1933, etc.). Column 3 further controls for for state of residence×year effects. This absorbs state-specific time-varying shocks or policies that may affect the probability of imprisonment and graduation. For example, an increase in prison spending in any given state may be offset by a decrease in education spending that year. (Notice, however, that since prison inmates may have committed their crime years before they are observed in prison, the state of residence×year effects are an imperfect control.) Both sets of estimates are insensitive to these additional controls.

The largest impact of education on imprisonment occurs when moving from less than high school to high school graduation (see Figure 1 or Table 2). Given the importance of high school completion in determining incarceration rates, we explore a specification in Table 4 that conditions on high school graduation rather than total years of schooling. We present results for the two most general specifications, identical to columns 2 and 3 of Table 3, although other specifications yield similar estimates. In this table, high school graduates include anyone with 12 or more years of schooling and a high school degree.[11] OLS estimates in columns 1 and 2 indicate that high school graduation is associated with a decrease in the probability of imprisonment by about 0.76 percentage points for whites and 3.4 percentage points for blacks.[12]

Two points about robustness are worth making now. First, in Section 5 we use NLSY data to show that these results are not sensitive to the inclusion of controls for individual ability and family background characteristics. In particular, models that include AFQT scores, family income, parents' education, whether or not the individual lived with both of his natural parents at age 14 and whether his mother was teenager at his birth estimated using NLSY data yield estimates that are remarkably similar to those based on Census data. Second, probit models yield similar estimated effects. We further examine the sensitivity of these results and our IV results below.

To help in interpreting the size of these impacts on incarceration, one can use these estimates to calculate how much of the black-white gap in incarceration rates is due to differences in educational attainment. In 1980, the difference in incarceration rates for whites and blacks is about 2.4%,

at the state of birth - year of birth level.

[11] We ignore the fact that in some years, high school graduation in South Carolina could be achieved with 11 years of schooling. We also ignore the fact that some inmates graduate in prison, which is uncommon in the years we examine. If some inmates graduate from high school while in prison, these estimates will be biased toward finding no effect of graduation on crime.

[12] Both coefficients are larger than the unconditional estimates, 0.06 percentage points for whites and 1.7 for blacks, obtained by simply differencing the average imprisonment rates. The difference between conditional and unconditional estimates is explained by the inclusion of age effects in the conditional specification. Drop out rates increase with age in our sample – due to the secular trend of increasing education throughout the century – and imprisonment rates decrease with age – since most crimes are committed by younger men.

Using the estimates for blacks, we conclude that 23% of the difference in incarceration rates between blacks and whites could be eliminated by raising the average education levels of blacks to the same level as that of whites.

Alternatively, consider that the rise in white and black high school graduation rates between 1970 and 1980 are 13 and 21 percentage points, respectively.[13] In this case, the rise in graduation rates among whites should have caused a decrease of 0.1 percentage points in their incarceration rates (compared to a base of 0.6% among white drop outs in 1970). The corresponding figure for blacks is 0.6 percentage points (compared to a base of 2.9% among black drop outs in 1970).

3.2 The Effect of Compulsory Attendance Laws on Schooling Achievement

The OLS estimates just presented are consistent with the hypothesis that education reduces the probability of imprisonment. If so, the effect appears to be statistically significant for both whites and blacks, and quantitatively larger for blacks. However, these estimates may reflect the effects of unobserved individual characteristics that influence the probability of committing crime and dropping out of school. For example, the theoretical model in Section 2 suggests that individuals with a high discount rate or taste for crime, presumably from more disadvantaged backgrounds, are likely to commit more crime and attend less schooling. To the extent that variation in unobserved discount rates and criminal proclivity across cohorts is important, OLS estimates could overestimate the effect of schooling on imprisonment.

It is also possible that juveniles who are arrested or confined to youth authorities while in high school may face limited educational opportunities. Even though we examine men ages 20 and older, some are likely to have been incarcerated for a few years, and others may be repeat offenders. If their arrests are responsible for their drop out status, this should generate a negative correlation between education and crime. Fortunately, this does not appear to be an important empirical problem.[14]

The ideal instrumental variable induces exogenous variation in schooling but is uncorrelated with discount rates and other individual characteristics that affect both imprisonment and schooling. We use changes over time in the number of years of compulsory education that states mandate as an instrument for education. Years of compulsory attendance are defined as the maximum between (i) the minimum number of years that a child is required to stay in school and (ii) the difference between the earliest age that he is required to be in school and the latest age he is

[13] These graduation rates refer to the proportion of 20-60 year old men with a high school degree and not the graduation rate of cohorts from those years.

[14] A simple calculation using NLSY data suggests that the bias introduced by this type of reverse causality is small. The incarceration gap between high school graduates and drop outs among those who were not in jail at ages 17 or 18 is 0.041, while the gap for the full sample is only slightly larger (0.049). Since the first gap is not affected by reverse causality, at most 10% of the measured gap can be explained away by early incarceration resulting in drop out. If some of those who were incarcerated would have dropped out anyway (not an unlikely scenario), less than 10% of the gap is eliminated.

required to enroll. Figure 2 plots the evolution of compulsory attendance laws over time for 49 states (all but Alaska and Hawaii). In the years relevant for our sample, 1914 to 1974, states changed compulsory attendance levels several times, and not always upward.

We assign compulsory attendance laws to individuals on the basis of state of birth and the year when the individual was 14 years old. To the extent that individuals migrate across states between birth and age 14, the instrument precision is diminished, though IV estimates will still be consistent. We create four indicator variables, depending on whether years of compulsory attendance are 8 or less, 9, 10, and 11 or 12.[15] The fractions of individuals belonging to each compulsory attendance group are reported in Table 1.

Figure 3 shows how the increases in compulsory schooling affect educational attainment over time, controlling for state and year of birth.[16] In the 12 years before the increase, there is no obvious trend in schooling achievement. All of the increase in schooling associated with stricter compulsory schooling laws takes place *after* changes in the law. This figure is important because it suggests that changes in compulsory schooling laws are not simply picking up underlying trends in education. Stricter compulsory attendance laws appear to raise education, not vice versa. More formal tests of reverse causality are provided below.

Table 5 quantifies the effect of compulsory attendance laws on educational achievement. These specifications include controls for age, year, state of birth, state of residence, and cohort of birth effects. To account for the impact of Brown v. Board of Education on the schooling achievement of Southern born blacks, they also include an additional state of birth dummy for black cohorts born in the South turning age 14 in 1958 or later. Identification of the estimates comes from *changes* over time in the number of years of compulsory education in any given state. The identifying assumption is that conditional on state of birth, cohort of birth, state of residence and year, the timing of the changes in compulsory attendance laws within each state is orthogonal to characteristics of individuals that affect criminal behavior like family background, ability, risk aversion, or discount rates.

Consider the estimates for whites presented in the top panel. Three points are worth making. First, the more stringent the compulsory attendance legislation, the lower is the percentage of high school drop outs. In states/years requiring 11 or more years of compulsory attendance, the number of high school drop outs is 5.5% lower than in states/years requiring 8 years or less (the excluded case). These effects have been documented by Acemoglu and Angrist (2000) and

[15] The data sources for compulsory attendance laws are given in Appendix B of Acemoglu and Angrist (2000). We use the same cut off points as Acemoglu and Angrist (2000). We experimented with a matching based on the year the individual is age 16 or 17, and found qualitatively similar results.

[16] The figure shows the estimated coefficients on leads and lags of an indicator for whether compulsory schooling increases in an individual level regression that also controls for state of birth and year of birth effects. The dependent variable is years of schooling. Lags include years -12 to -3. Leads include years +3 to +12. Time 0 represents the year the respondent is age 14.

Lleras-Muney (2000).[17] Second, the coefficients in columns 1 and 2 are roughly equal, but with opposite sign. For example, in states/years requiring 9 years of schooling, the share of high school drop outs is 3.3 percentage points lower than in states/years requiring 8 years or less of schooling; the share of high school graduates is 3.3 percentage points higher. This suggests that compulsory attendance legislation does reduce the number of high school drop outs by 'forcing' them to stay in school. Third, the effect of compulsory attendance is smaller, and in most cases, not significantly different from zero in columns 3 and 4. Finding a *positive* effect on higher levels of schooling may indicate that the laws are correlated with underlying trends of increasing education, which would cast doubt on their exogeneity. This does not appear to be a problem in the data. The coefficient on compulsory attendance ≥ 11 for individuals with some college is negative, although small in magnitude, suggesting that states imposing the most stringent compulsory attendance laws experience small declines in the number of individuals attending community college. This result may indicate a shift in state resources from local community colleges to high schools following the decision to raise compulsory attendance laws.

The bottom panel in Table 5 reports the estimated effect of compulsory attendance laws on the educational achievement of blacks. These estimates are also generally consistent with the hypothesis that higher compulsory schooling levels reduce high school drop outs rates, although the coefficients in column 1 are not monotonic as they are for whites. The coefficients in column 3 are negative, suggesting that increases in compulsory attendance are associated with decreases in the percentage of black men attending local colleges. The magnitudes are smaller than the effect on high school graduation rates but larger than the corresponding coefficients for whites. This may reflect a shift in resources from local black colleges to white high schools, and to a lesser extent, to black high schools.[18] As expected, compulsory attendance laws have little effect on college graduation.

Are compulsory schooling laws valid instruments? We start to address this question by examining whether increases in compulsory schooling ages are associated with increases in state resources devoted to fighting crime. If increases in mandatory schooling correspond with increases in the number of policemen or police expenditures, IV estimates might be too large. However, we do not expect this to be a serious problem.

First, in contrast to most studies using state policy changes as an instrument, simultaneous changes in compulsory schooling laws and increased enforcement policies are not necessarily problematic for the instrument in this study, since we examine incarceration among individuals

[17] Having a compulsory attendance law equal to 9 or 10 years has a significant effect on high school graduation. Possible explanations include "lumpiness" of schooling decisions (Acemoglu and Angrist, 2000), educational sorting (Lang and Kropp 1986), or peer effects.

[18] To the extent that compulsory attendance laws reduce college attendance, IV estimates will be biased toward finding no effect (or even a positive effect) of high school graduation on crime.

many years after schooling laws are changed and drop out decisions are made. Recall that we assign compulsory attendance based on the year an individual is age 14, and our sample only includes individuals ages 20 and older. For the instrument to be invalid, state policy changes that take place when an individual is age 14 must directly affect his crime years later (in his twenties and thirties). In general, this does not appear to be a likely scenario. However, as an additional precaution, we absorb time-varying state policies in our regressions by including state of residence×year effects.

Second, we directly test for whether increases in compulsory attendance laws are associated with increases in the amount of police employed in the state. We find little evidence that higher compulsory attendance laws are associated with greater police enforcement. Column 1 in Table 6 reports the correlation between the instruments and the per capita number of policemen in the state. Data on policemen are from the 1920 to 1980 Censuses. Columns 2 and 3 report the correlation between the instruments and state police expenditures and per capita police expenditures, respectively, using annual data on police expenditures from 1946 to 1978.[19] No clear pattern emerges from columns 1 and 2, while there appears to be a negative correlation in column 3. Overall, we reject the hypothesis that higher compulsory attendance laws are associated with an increase in police resources. If anything, per capita police expenditures may have *decreased* slightly in years when compulsory attendance laws increased (consistent with trade-offs associated with strict state budget constraints).

Another important concern with using compulsory attendance laws as an instrument is that the cost of adopting more stringent versions of the laws may be lower for states that experience faster increases in high school graduation rates. It is, therefore, possible that changes in compulsory attendance laws simply reflect underlying state-specific trends in graduation rates. We have already shown in Figure 3 that increases in education follow increases in compulsory schooling, and that in the 12 years prior to the increase there is no observable trend in schooling. We now quantify the relationship between future compulsory attendance laws and current graduation rates. If causality runs from compulsory attendance laws to schooling, we should observe that future laws do not affect current graduation rates conditional on current compulsory attendance laws. Results of this test are reported in Table 7. The coefficients in the first row, for example, represent the effect of compulsory attendance laws that are in place 4 years after individuals are age 14. All models condition on compulsory attendance laws in place when the individual is age 14, 15, 16, and 17 (these coefficients are not reported but are generally significant). To minimize problems with multicollinearity, we run separate regressions for each future year (i.e. each row is a separate regression), although results are similar when we run a single regression of compulsory attendance on all future years. Positive coefficient estimates on future schooling laws would be

[19] Data on police expenditures are taken from ICPSR Study 8706.

consistent with causality running from schooling to compulsory attendance, and would cast doubt on our identifying assumption. Overall, the results in Table 7 suggest that states with faster expected increases in graduation rates are not more likely to change their compulsory attendance laws.[20] This result is consistent with the findings of Lleras-Muney (2000) who examines these laws from 1925-39.

3.3 Instrumental Variable Estimation

We now present instrumental variable estimates of the impact of schooling on the probability of incarceration. Ideally, one would like to estimate an unrestricted model of incarceration on a full set of schooling dummies (analogous to the regressions underlying Figure 1) using IV to avoid any endogeneity problems. Unfortunately, this is not feasible, since the range of compulsory schooling ages is quite limited. In fact, there is too little variation in the laws to even identify a model of incarceration that is generally linear in schooling but that also allows for a separate effect of high school completion. Instead, when we move to IV techniques we are limited to estimating models that either only include an indicator for high school completion or are linear in schooling.

We estimate models identical to our earlier OLS specifications using 2SLS, and begin with a discussion of the linear-in-schooling models. Returning to Table 3, the 2SLS estimates in columns 1-6 suggest that one extra year of schooling reduces the probability of imprisonment by about .1 percentage points for whites and .3-.5 percentage points for blacks. These estimates are stable across specifications and nearly identical to the corresponding OLS estimates. This indicates that the endogeneity bias is unlikely to be quantitatively important after controlling for age, time, state of residence, and state of birth. We cannot reject that the OLS and 2SLS estimates are the same using a standard Hausman test. The table also reports the test statistics for F-tests of whether the compulsory schooling attendance laws all have zero coefficients.

A slightly different story emerges when we move to Table 4, which examines the effect of high school graduation on the probability of incarceration. In general, the estimated effect of graduation on the probability of imprisonment among white men is stable around -0.6% to -0.9% across both 2SLS specifications, quite similar to the corresponding estimates using OLS. However, the 2SLS estimated effects for blacks range from -7% to -8%, roughly twice the corresponding OLS estimates. While endogeneity issues would lead us to expect OLS estimates to be biased toward finding too large an effect, the OLS estimates are actually smaller than the 2SLS estimates for blacks. Why? The answer lies in understanding that OLS and 2SLS do not necessarily estimate the same parameter of interest. When estimating the impact of high school graduation on the

[20] Only one estimated coefficient for whites is significantly positive ($t = +1.8$). The only significant positive coefficients for blacks refer to laws 15 or more years in the future, too far ahead to be comfortably interpreted as causal. Furthermore, for those years where the coefficients are positive, there is no relationship between stringency of the law and high school drop out, making it difficult to interpret this finding.

probability of incarceration using only an indicator variable for graduation status. OLS and 2SLS estimators can be written as weighted sums of causal responses to each unit change in schooling (i.e. completing grades 9, 10, 11, 12, etc.), where the sets of "weights" differ for the two estimators. The expected difference between the estimators depends on the difference in the weights as well as the impacts of schooling on incarceration at each grade level. In Appendix A, we discuss this issue in detail and empirically show that differences in these "weights" explain much of the difference between our OLS and 2SLS estimates. Moreover, the differential "weights" explain why the OLS and 2SLS estimates are similar for whites and not for blacks when using the graduate dummy specification. After taking these factors into account, the instrumental variable estimates for high school graduation confirm the OLS results.[21]

Overall, we find strong evidence that education reduces the probability of incarceration. Both OLS and 2SLS estimates agree, suggesting that endogeneity problems are relatively unimportant after controlling for age, year, state of residence, and state of birth. An additional year of schooling reduces the probability of incarceration by about .1 percentage point for whites and .4 percentage points for blacks. The estimated impact of high school graduation is about 0.8 percentage points for whites and 3.4 percentage points for blacks.

In addition to the effects of years of schooling and high school graduation on imprisonment, one may also be interested in the effect of moving from 11 to 12 years of schooling. This effect may be important for policy reasons, given the substantial interest in programs that encourage youth to finish high school. Based on Figure 1, the greatest reductions in the probability of incarceration are associated with the final year of high school: the effect is 0.6% for whites and 2.2% for blacks. In the Appendix, we build on Angrist and Imbens (1995) to show that the causal effect of finishing 12th grade is bounded by the 2SLS estimates from the linear-in-schooling (Table 3) and high school graduate (Table 4) specifications.

In Table 8, we explore the robustness of the estimated effects of an additional year of schooling on the probability of incarceration. All specifications control for age, year × state of residence, state of birth, and cohort of birth. Specification A reports the base case results from Table 3 for ease of comparison. The following three models aim at absorbing trends that are specific

[21] For completeness, we explore two other potential explanations for the difference between OLS and 2SLS estimates for blacks (when using the specification with a dummy for graduation), but we find little empirical support for either of them. First, we study whether heterogeneity in the rates of return to schooling can explain the discrepancy given that 2SLS estimates of the Mincerian rate of return to schooling are typically greater than OLS estimates (Card 1995). We find that IV estimates are indeed larger than OLS estimates, but they are larger for both blacks and whites. Second, we study whether spillovers or contagion effects, which have been suggested by Glaeser, et. al (1996) and Gaviria and Raphael (2001), may be responsible for the difference in estimates. If individual decisions to commit crime depend on average education levels or crime rates for other individuals in their cohort and state, 2SLS (using state-cohort level instruments as we do) will estimate the sum of the individual effect and spillover effect. If cross-state and cohort variation in average graduations rates are small relative to overall variation in graduation rates, then OLS will only estimate the individual effect of graduation on crime. Empirically, this does not appear to be an important factor.

to the region or the state of birth to account for geographic differences in school quality over time, as well as differences in other time-varying factors that are specific to the state of birth and correlated with schooling. Specification B includes region of birth specific linear trends in year of birth. Specification C includes the interaction of region of birth effects and cohort of birth effects. Specification D further relaxes the model by allowing for different trends in cohort quality at the state level.

These three specification come close to fully saturating the model. For example, in specification D the 2SLS estimator is identified only by deviations of compulsory attendance laws from a linear trend. The loss of identifying variation in the first stage is indicated by the drop in reported first stage F-test statistics. OLS estimates are unchanged. While the 2SLS estimates show greater effects, they are much less precise and statistically indistinguishable from the base case estimates.

Specification E allows the cohort effects to vary with age, capturing the possibility that age-crime patterns have varied over time. Estimates are similar to the base case.

Finally, specification F allows the impact of education on the probability of incarceration to vary with age. Ideally, one would like to split the sample into two or three age groups, running separate regressions for each group. However, there is not enough variation in the data to obtain precise IV estimates separately for each age group. The estimates of model F suggest that the effects are larger for younger men, declining with age. In addition to the coefficient estimates, we report the implied effects at ages 20 and 40. Among white men, the OLS estimates suggest that an additional year of schooling reduces the probability of incarceration by about 0.11 percentage points at age 20 and by 0.06 percentage points at age 40. The corresponding estimates for blacks imply an effect of 0.6 and 0.4 percentage points at ages 20 and 40, respectively.

4 The Impact of Schooling on Arrest Rates

One limitation of Census data is that they do not differentiate among different types of criminal offenses. In this section, we investigate the impact of education on specific crime rates by using data on arrests by offense. Because individual-level data that contain education of the arrested do not exist, we use arrest data collected by the FBI Uniform Crime Reports (UCR) by state, criminal offense, and age for 1960, 1970, 1980, and 1990. For each year and reporting agency, arrests are reported by age group, gender, and offense type. Unfortunately, arrest rates are not reported by race in addition to state, age, and year. We only study males ages 20-59 in our analysis.

To relate arrest rates to schooling and racial composition, we augment the arrest data with average education levels and high school graduation rates by age and state as well as the percentage

black by age in each state from the 1960-1990 Censuses. We estimate the following model:

$$\ln A_{cast} = \beta E_{ast} + \gamma B_{ast} + d_{st} + d_{sc} + d_{sa} + d_{ct} + d_{at} + d_{ac} + e_{cast} \tag{2}$$

where $\ln A_{cast}$ is the logarithm of the male arrest rate for crime c, age group a, in state s in year t (from UCR); E_{ast} is either average education or the high school graduation rate for males in age group a in state s at time t (from Census); B_{ast} is the percent of males that are black in age group a in state s at time t (from Census). In using log arrest rates, the effect of education on arrest rates is assumed to be the same *in percentage terms* for all crimes.[22] In a few specifications, we allow the effect of schooling to vary by type of crime (β_c).

The d's represent indicator variables that account for unobserved heterogeneity across states, years, cohorts, and criminal offense types. In particular, d_{st} is a state×year effect that absorbs time varying, state-specific shocks that may induce spurious correlation. The level of arrests reflects both the level of criminal activity and police resources devoted to making arrests. If a state decides to reduce spending for public education and increase spending for police or prisons, a spurious positive correlation between arrests and schooling may arise. Including state-year effects is more robust than including observable state-level variables reflecting differences in spending or punishment. Since for each state-year combination there are many age groups in our data, we can control for unrestricted state-specific time-varying shocks without fully saturating the model. For example, average schooling and arrest rates of men ages 20-24 are different from average schooling and arrest rates of men ages 25-29 in the same state and year.

In estimating equation (2), the distribution of crimes across states does not need to be uniform. Some states may focus arrests more heavily on some types of crimes than others, either because more of those crimes are committed or because that state is simply harsher on those crimes. Also, the age of arrestees need not be the same across states – some age groups may be more prone to commit crimes in some states or the arrest policy with respect to age may differ across states. The terms d_{sc} and d_{sa} absorb permanent state×crime and state×age heterogeneity in arrest rates. Crime-specific and age-specific trends in arrest common to all states are accounted for by crime×year dummies, d_{ct}, and age×year dummies, d_{at}, respectively. Finally, age×crime effects, d_{ac}, account for the fact that some age groups might always be more likely to commit certain types of crimes and to be arrested for those crimes. In the data, we have 8 age groups (20-24, 25-30, etc.), 9 crimes (murder, rape, assault, robbery, burglary, larceny, auto theft, and arson), and 51 states.

Most crimes do not result in an arrest. We are interested in arrests, however, because there is presumably a link between the amount of crime that takes place and the number of arrests that are made. To establish that link, we first compare our arrest data with crime reported

[22] This assumption is consistent with that made by Levitt (1998). We have also estimated specifications in arrest rates (rather than log arrest rates) and arrived at similar conclusions.

to the police in the FBI's Uniform Crime Reports. The crime reported to the police in the UCR is used by the FBI to calculate official crime rates. The average arrest-crime ratio across all years and states is 0.6 for murder and declines substantially as we move toward less serious crimes. Although this fact suggests that very few arrests are made for each crime committed, the correlation between arrests and crimes committed is remarkably high: 0.97 for burglary, 0.96 for rape and robbery, 0.94 for murder, assault and burglary, and 0.93 for motor vehicle theft. This suggests that variation in arrest rates closely tracks variation in actual crimes committed.[23]

The estimated impacts of education on arrest rates are reported in Table 9. The top half reports the effects of average education levels and the bottom half reports the effects of high school graduation rates. Columns 1-3 report OLS estimates, and columns 4-6 report 2SLS estimates using compulsory schooling laws as instruments. We assign the compulsory attendance laws based on the state where the arrest took place and the year the arrestees were age 14.[24] All models are weighted by cell size. Since variation in arrest rates occurs across offense type, age, state, and year, and variation in graduation rates occurs across age, state, and year, standard errors are corrected for state-year-age clustering.

The OLS estimates suggest that a one-year increase in average education levels is estimated to reduce arrest rates by 11%. 2SLS estimates suggest slightly larger effects, although they are not statistically different. While the standard errors more than double when using 2SLS, the estimates are still generally statistically significant. Given the importance of high school completion in determining incarceration rates, we also explore the relationship between high school graduation rates and arrest rates in the bottom half of the table. The OLS estimated impacts of high school graduation rates range from 0.6-0.7, while 2SLS estimates suggest a larger effect (though they are less precisely estimated).

Table 10 allows for differential effects of schooling across different types of crime. The top half distinguishes between violent and property crimes, while the bottom half examines arrests for more detailed types of crimes. In interpreting these results, recall that when an individual is arrested for committing more than one crime, only the most serious is recorded. For example, if a murder is committed during a burglary, the arrest is recorded as murder. This may blur the distinction between violent and property crime. Estimates for years of schooling are in columns 1 and 2. The upper panel shows similar effects across the broad categories of violent

[23]Levitt (1998) transforms arrest rates into implied crime rates using the following algorithm: $Crime_{ast} = Arrest_{ast} * (Crime_{st}/Arrest_{st})$ under the assumption that the number of crimes committed by a cohort in a given state and year is proportional to that cohort's share of total arrests in that state and year. Since we use the *logarithm* of arrests, and we control for state*year effects, our specification is similar to Levitt's (1998). (They would be identical if we studied only one type of crime.)

[24]Unfortunately, we cannot assign compulsory attendance directly to individuals as we could with the Census data. Nor can we assign compulsory attendance based on the *state of birth*, since it is not available in the FBI aggregate data. Because of these data limitations, we expect a decrease in precision. Still the first stage estimated effects of compulsory schooling laws on education are significant.

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and property crime; however, the bottom panel suggests that the effects vary considerably within these categories. A one year increase in average years of schooling reduces murder and assault by almost 30%, motor vehicle theft by 20%, arson by 13%, and burglary and larceny by about 6%. Estimated effects on robbery are negligible, while those for rape are significantly positive. This final result is surprising and not easily explained by standard economic models of crime.[25]

We find very similar patterns when looking at the relationship between high school graduation rates and arrest rates, reported in columns 3 and 4. The estimates for detailed arrests imply that a ten percentage point increase in graduation rates would reduce murder and assault arrest rates by about 20%, motor vehicle theft by about 13%, and arson by 8%.[26]

Because arrest rates are not reported by race in addition to state, age, and year, it is difficult to determine whether schooling has differential effects on arrest by race. We attempt to examine this issue by controlling for both the schooling levels of blacks and whites in each state. To do this, we interact black (and white) educational attainment by age and state with the fraction of men who are black (and white) in that same age and state category. If total arrests are the sum of arrests for blacks and for whites, then coefficients on these variables will give us the impacts of education on arrests for each race. We find some evidence that the impact is greater for blacks.[27]

As a whole, these results suggest that schooling is negatively correlated with many types of crime even after controlling for a rich set of covariates that absorb heterogeneity at the state, year, crime, and age level. Both IV and OLS estimates are similar, again suggesting that endogeneity problems are empirically unimportant.

Are these estimates consistent with the Census-based incarceration estimates of the previous section? As discussed in Section 2, if sentence lengths or the probability of incarceration given arrest are greater for less educated individuals, the log difference in incarceration rates by education should exceed the log difference in arrest rate by the log difference in the probability of

[25]We originally thought that it may be explained by differential reporting rates by education, with more educated women more likely to report a rape. To test this hypothesis we examined reporting rates from the National Criminal Victimization Survey, but we failed to find evidence of such differential reporting. It is still possible that less educated women tend to be more restrictive in their definition of rape.

[26]High school graduation rates appear to have a slightly larger effect on violent crimes (especially murder and assault) than property crimes. This may be surprising since one channel through which schooling can affect crime is through raising wage rates and, therefore, the opportunity costs of crime. But, it is consistent with the fact that punishments for violent crimes typically involve substantially longer prison sentences, which are more costly when wages and schooling are high. And, to the extent that schooling increases patience levels or risk aversion, the long prison sentences associated with violent crimes become more costly. Non-economic factors may also play an important role in determining criminal activity. For example, finishing high school may cause individuals to change their lifestyles, residential location, or peer groups, reducing the amount of criminal opportunities they come into contact with and choose to engage in. Finally, the large coefficients on murder and assault may, in part, reflect the fact that only the most serious crime gets reported by the FBI when multiple crimes are committed.

[27]For example, in a specification analogous to that of column 2 in the bottom panel of Table 9, the coefficient estimate for the interaction of black graduation rates with percent black and violent crime is -2.49 (0.49), while it is -1.50 (0.49) for property crime. The corresponding estimates for whites are only -0.38 (0.24) and -0.31 (0.25). When we also control for state-specific year effects as in column (3) of Table 9, the lack of race-specific arrest rates makes precise estimation of race-specific graduation impacts difficult.

incarceration given arrest. Since Mustard (2001) finds differences of only 2-3% in sentencing by graduation status, we should expect comparable effects of education on log arrest rates and log incarceration rates. The log difference in incarceration rates between high school drop outs and graduates for all men in the Census is about 1.4 (IV estimates produce larger impacts for blacks). The IV estimates in Table 9, obtained using data on all offenses, suggest that graduation reduces arrest rates among all men by nearly 1 log point. OLS estimates suggest an overall effect of about 0.7 log points, while crime-specific estimates suggest effects as large as 2.2 log points for violent crimes (carrying a long prison sentence) such as assault and murder. These simple comparisons suggest that the estimated effects on arrest and incarceration rates are roughly consistent.

One might also expect effects of this magnitude based on the estimated impact of increased wage rates on crime and arrest rates. For example, Grogger (1998) estimates an elasticity of criminal participation with respect to wages of around 1-1.2 using self-report data from the NLSY. Gould, et. al (2000) estimate the elasticity of arrest rates to the local wage rates of unskilled workers to be in the neighborhood of 1-2. When using March CPS data from 1964-90, a standard log wage regression controlling for race, experience, experience-squared, year effects, and college attendance yields an estimated coefficient on high school graduation of 0.49. Combining this estimate of the effect of schooling on wages with the elasticity of arrests with respect to wages estimated by Gould, et. al (2000) produces an impact of 0.5-1.0. That is, a 10% increase in high school graduation rates should reduce arrest rates by 5-10% through increased wages alone. This covers the range of estimates in Tables 9 and 10 and confirms that an important explanation for the effect of high school graduation on crime resides in the higher wage rates associated with finishing high school.

5 The Impact of Schooling on Criminal Participation and Incarceration in the NLSY

Since crime is not directly observed, we have used data on arrests and incarceration to estimate the impacts of education on crime. Those results suggest that schooling is associated with a lower probability of arrest and imprisonment. Because those estimates may confound the effects of schooling on actual crime with any educational differences in the probability of arrest or incarceration conditional on commission of a crime (see Section 2), we turn to the National Longitudinal Survey of Youth to study the relationship between education and self-reported crime. Although self-reported crime may suffer from under-reporting, it is the most *direct* measure of criminal participation available.

The NLSY also offers an abundance of individual-level variables that may determine crime but which are not available in the Census or arrest data we have used thus far. Therefore, a second important advantage of the NLSY is that it can be used to determine the robustness of our earlier

results to the inclusion of more control variables likely to be related to crime. In particular, the survey records scores on the Armed Forces Qualifying Test (AFQT) that can be used as a measure of cognitive ability. Parents' age and education are available, as is family income. The NLSY also indicates whether or not individuals lived with both of their natural parents at age 14 and whether the mother was a teenager when she gave birth. Because the NLSY follows respondents who become incarcerated, we are able to verify our Census-based findings in Section 3.

We create three self-reported crime categories corresponding to more serious offenses: (i) property crimes consist of thefts greater than or equal to $50 as well as shop-lifting; (ii) violent crimes consist of using force to get something or attacking with intent to injure or kill (i.e. robbery and assault); and (iii) drug crimes consist of selling marijuana or hard drugs. Individuals are considered to be incarcerated if (i) they were surveyed in prison or (ii) they reported incarceration as a reason they were not looking for work when they were unemployed during the survey year (post-1988 only).

While it is virtually impossible to verify self-reported crime, most studies agree that young black men are more likely to under-report their criminal behavior than young white men. (See for example the exhaustive study by Hindelang, Hirsch, and Weis (1981).) Our calculations based on NLSY data suggest that black drop outs may be substantially under-reporting criminal activity, while there is less reason to believe that black high school graduates and whites are under-reporting to the same degree.[28] Because a correlation between under-reporting and education would bias any estimates of the impact of schooling on crime, we focus attention on white men in the NLSY.

Table 11 reports the estimated effects of schooling on self-reported criminal participation and incarceration among young white men in the NLSY using OLS. Self-reported crime measures are for men ages 18-23 in 1980, while incarceration measures represent the annual rate of incarceration over ages 22-28. Two goals are pursued. First, we examine the impacts of schooling on self-reported crime to compare with the results for arrests and incarceration. Second, to determine the robustness of our findings, we explore much richer specifications that control for family background, individual ability, and local labor markets.

We begin with sparse specifications analogous to those used in the previous sections, control-

[28] Among black drop outs, the self-reported crime rate at ages 18-23 is 0.22, but the incarceration rate over ages 22-28 is 0.32. While self-reported criminal activity may suffer from under-reporting, the incarceration data are reliable, since they are primarily based on whether the respondent is interviewed in prison. Given that crime typically declines with age among adults and 32% of the black high school drop outs in the sample were incarcerated over ages 22-28, it seems highly unlikely that only 22% of young black drop outs participated in crime just a few years earlier. In the absence of gross incarceration of innocent black men, it is likely that black drop outs substantially under-reported their criminal involvement in the NLSY. Among whites and black graduates, self-reported crime rates are more consistent with subsequent incarceration rates. As a result, differential reporting by educational attainment is likely to be less of a problem among whites. More accurate reporting among whites accords with previous studies (Hindelang, Hirsch and Weis 1981).

ling for age and region of residence. Because the sample is so young and many of the men are still in school, we also control for school enrollment. As indicated by columns 1 and 3, both years of schooling and high school graduation significantly reduce participation in violent, property, and drug crimes. An additional year of school reduces participation in each type of crime by around 2-3 percentage points. High school graduation reduces participation rates in violent crime by 0.11, drug sales by 0.06, property crime by 0.13, and overall criminal participation by 0.18.

Columns 2 and 4 control for age, family background,[29] ability (as measured by AFQT percentile), race and ethnicity, geographic location (region of residence and SMSA status), local unemployment rates, and statewide incarceration rates (the ratio of prisoners to crimes committed in the state as taken from Levitt (1998)). (In estimating the effect of high school graduation in column 4, we also condition on college attendance, which shows no significant effects.) The striking result is that these estimates obtained by conditioning on a rich set of individual and family background characteristics are nearly identical to the parsimonious specifications used throughout the paper. In other words, ignoring cognitive ability and family background does not introduce an upward bias in estimating the effect of high school graduation on criminal participation.

How do these effects compare with our findings for arrest rates? We compare arrest results from Table 10 with the log difference in self-reported crime by high school graduation status in the NLSY. The difference in self reported log violent crime rates is 0.92, slightly larger than the measured effect on violent arrests, 0.79. The difference in self-reported log property crime rates is 0.43, slightly less than the estimated effect on property arrests, 0.62. These findings suggest that the estimated impacts of graduation on arrests and incarceration are not simply the result of differential treatment by police and judges. Education has a real effect on crime that is measurably similar to its effects on both arrest and incarceration.[30] This reconciles with the finding of Mustard (2001) that average prison sentences are quite similar across high school graduates and drop outs.

We next examine the impact of education on incarceration in the NLSY to verify our earlier results using Census data. The estimated effects of schooling on incarceration during early adulthood are shown in the bottom row of Table 11. As in Section 3, education significantly reduces the probability that a young man will be incarcerated. Estimates for both years of schooling and high school graduation are similar across the parsimonious and rich specifications, suggesting that an additional year of schooling reduces the annual probability of incarceration by about 0.5 percentage points and that high school graduation reduces the probability by about 3 percentage

[29]Family background measures include: current enrollment in school, parents highest grade completed, whether or not the individual lived with both of his natural parents at age 14, whether his mother was a teenager at his birth, and family income.

[30]It should be noted that self-report estimates measure the effects on criminal participation at the extensive margin, so they need not correspond perfectly to arrest rates, which include changes at the intensive and extensive margin.

points among white men ages 22-28.[31] While these estimated effects are larger than those found with the Census data, the discrepancy is explained by the fact that the Census estimates report incarceration rates over ages 20-60, while the NLSY-based estimates refer to men ages 22-28. When annual incarceration rates are compared for 22-28 year-old men, both data sources yield remarkably similar predictions.[32]

Two points are evident from the NLSY data. First, education significantly reduces self-reported crime, and the estimated effects are consistent with the impacts estimated for arrests and incarceration in Sections 3 and 4. This implies that the impacts estimated for arrests and incarceration reflect a true effect on crime, and not simply educational differences in the probability of arrest or incarceration conditional on commission of a crime. Second, controlling for individual ability, family background, and local labor markets does not change the estimated effect.

6 Social Savings from Crime Reduction

Given the estimated impact of education on crime, it is possible to determine the social savings associated with increasing education levels. Because the social costs of crime differ substantially across crimes, we use estimates based on the impact of schooling on arrests by offense type to determine the social benefits of increased education. Recognizing that the effects of schooling tend to be more important during the high school years (particularly at the 12th grade level) and due to the substantial policy interest in high school drop out, we estimate the social benefits through reduced crime of increasing the high school graduation rate by 1%.

These estimates are subject to two important caveats. First, they assume that estimates in Table 10 produce a consistent estimate of the effect of graduation on arrest. Second, consistent with most other studies of crime, these estimates do not account for general equilibrium effects on wages resulting from an increase in the supply of graduates. However, in Lochner and Moretti (Appendix B, 2001), we present a simple general equilibrium model to assess how sensitive our estimates of social savings might be to the inclusion of general equilibrium effects. The intuition of the model is very simple. An increase in the supply of high school graduates reduces their wage levels which should increase their crime rate. This would suggest that our social benefit calculations overestimate the true social savings. At the same time, however, a reduction in the supply of drop outs increases their wage rates which should decrease their crime rate causing us to understate the true social savings. A back of the envelope calculation reported in Lochner and

[31] These estimates adjust the impact of graduation on the probability of incarceration over the entire age span of 22-28 to an annual impact using the ratio of incarceration rates over the seven year period to the annual incarceration rate (a factor of 3). Though not shown, results for blacks are also consistent with Census results and are available from the authors upon request.

[32] For example, annual incarceration rates among males ages 22-28 in the Census are 0.025 and 0.005 for white drop outs and graduates, respectively.

Moretti (Appendix B, 2001) suggests that the net effect of changing wages on crime is trivial. If anything, when 1% of the population is moved from dropout to graduate status, the reduction in wages among graduates is more than offset by the increase in wages among drop outs, so that the net effect on crime when general equilibrium effects are included is no smaller than what is reported here.

Recognizing the limitations of the exercise, we nonetheless provide a rough estimate of the social savings from crime reduction resulting from a 1% increase in high school graduation rates. Columns 1 to 4 of Table 12 report the costs per crime associated with murder, rape, robbery, assault, burglary, larceny/theft, motor vehicle theft, and arson. Victim costs and property losses are taken from Miller, et al. (1996). Victim costs reflect an estimate of productivity and wage losses, medical costs, and quality of life reductions based on jury awards in civil suits. Incarceration costs per crime equal the incarceration cost per inmate multiplied by the incarceration rate for that crime (approximately $17,000).[33] Total costs are computed by summing incarceration costs and victim costs less 80% of property losses, which are already included in victim costs and may be considered a partial transfer to the criminal.[34] The table reveals substantial variation in costs across crimes: violent crimes like murder and rape impose enormous costs on victims and their family members, while property crimes like burglary and larceny serve more to transfer resources from the victim to the criminal.

It is important to recognize that many costs of crime are not included in this table. For example, the steps individuals take each day to avoid becoming victimized — from their choice of neighborhood to leaving the lights on when they are away from home — are extremely difficult to estimate. More obvious costs such as private security measures are also not included in Table 12. Even law enforcement (other than costs directly incurred when pursuing/solving a particular crime) and judicial costs are absent here, mostly because they are difficult to attribute to any particular crime. Finally, the costs of other crimes not in the table may be sizeable. Nearly 25% of all prisoners in 1991 were incarcerated for drug offenses, costing more than $5 billion in jail and prison costs alone (Lynch et al. 1994). Given the NLSY findings for the effects of high school graduation on drug offenses, there is good reason to believe these costs of crime are also relevant for this analysis.

Column 5 reports the predicted change in total arrests in the U.S. based on the arrest estimates reported in panel B, column 4 of Table 10 and the total number of arrests in the Uniform Crime

[33] Incarceration rates by offense type are calculated as the total number of individuals in jail or prison (from Lynch, et al. (1994)) divided by the total number of offenses that year (where the number of offenses are adjusted for non-reporting to the police). Incarceration costs per inmate are taken from Stephan (1999). Offenses known to the police and reporting rates are given by the Uniform Crime Reports and National Criminal Victimization Survey.

[34] For the crime of arson, total costs equal victim costs plus incarceration costs, since it is assumed that none of the property loss is transferred to the criminal.

Reports. Our estimates imply that nearly 400 fewer murders and 8,000 fewer assaults would have taken place in 1990 if high school graduation rates had been one percentage point higher. Column 6 adjusts the arrest effect in column 5 by the number of crimes per arrest. In total, nearly 100,000 fewer crimes would take place. The implied social savings from reduced crime are obtained by multiplying column 4 by column 6 and are shown in column 7. Savings from murder alone are as high as $1.1 billion. Savings from reduced assaults amount to nearly $370,000. Because our estimates suggest that graduation increases rape and robbery offenses, they partially offset the benefits from reductions in other crimes. The final row reports the total savings from reductions in all eight types of crime. These estimates suggest that the social benefits of a one percent increase in male U.S. high school graduation rates (from reduced crime alone) would have amounted to $1.4 billion. And, these calculations leave out many of the costs associated with crime and only include a partial list of all crimes. Given these omissions, $1.4 billion should be viewed as an under-estimate of the true social benefit.

One might worry that our large estimated effects for murder combined with the high social costs of murder account for most of the benefits. When we, instead, use the estimated effects for violent and property crime in the top panel of Table 10, the resulting total social benefits from crime reduce to $782 million. (An overly conservative estimate that only considered savings from reductions in incarceration costs would yield a savings of around $50 million.)

The social benefit *per additional male graduate* amounts to around $1,170-$2,100, depending on whether estimates in the top or bottom panel of Table 10 are used. To put these amounts into perspective, it is useful to compare the private and social benefits of completing high school. Completing high school would raise average annual earnings by about $8,040.[35] Therefore, the positive externality in crime reduction generated by an extra male high school graduate is between 14% and 26% of the private return to high school graduation. The externalities from increasing high school graduation rates among black males are likely to be even greater given the larger estimated impacts on incarceration and arrest rates among blacks.

For another interesting comparison, consider what a one percent increase in male graduation rates entails. The direct costs of one year of secondary school were about $6,000 per student in 1990. Comparing this initial cost with $1,170-$2,100 in social benefits per year thereafter reveals the tremendous upside of completing high school.[36]

How do these figures compare with the deterrent effects of hiring additional police? Levitt

[35] This is based on a regression of log earnings on dummies for high school completion, college attendance, and other standard controls using males in the 1990 Census. The coefficient on the high school dummy, 0.42, was multiplied by $19,146, the average earnings for male workers with 10 or 11 years of schooling in the 1990 Census.

[36] Because the arrest estimates reflect the average difference between all high school graduates and all drop outs (rather than comparing those with 12 versus 11 years of schooling), the estimated benefits are likely to be greater than the benefits that result from simply increasing the schooling of those with eleven years by one additional year. However, as Figure 1 reveals, 70% of the reductions seem to be associated with finishing the final year of high school.

(1997) argues that an additional sworn police officer in large U.S. cities would reduce annual costs associated with crime by about $200,000 at a public cost of roughly $80,000 per year. To generate an equivalent social savings from crime reduction would require graduating 100 additional high school students for a one-time public expense of around $600,000 in schooling expenditures (and a private expense of nearly three times that amount in terms of foregone earnings). Of course, such a policy would also raise human capital and annual productivity levels of the new graduates by more than 40% or $800,000 based on our estimates using standard log wage regressions. So, while increasing police forces is a cost effective policy proposal for reducing crime, increasing high school graduation rates offers far greater benefits when both crime reductions and productivity increases are considered.

7 Conclusions

There are many theoretical reasons to expect that education reduces crime. By raising earnings, education raises the opportunity cost of crime and the cost of time spent in prison. Education may also make individuals less impatient or more risk averse, further reducing the propensity to commit crimes. To empirically explore the importance of the relationship between schooling and criminal participation, this paper uses three data sources: individual-level data from the Census on incarceration, state-level data on arrests from the Uniform Crime Reports, and self-report data on crime and incarceration from the National Longitudinal Survey of Youth.

All three of these data sources produce similar conclusions: schooling significantly reduces criminal activity. This finding is robust to different identification strategies and measures of criminal activity. The estimated effect of schooling on imprisonment is consistent with its estimated effect on both arrests and self-reported crime. Both OLS and IV estimates produce similar conclusions about the quantitative impact of schooling on incarceration and arrest. The estimated impacts on incarceration and self-reports are unchanged even when rich measures of individual ability and family background are controlled for using NLSY data. Finally, we draw similar conclusions using aggregated state-level UCR data as we do using individual-level data on incarceration and self-reported crime in the Census or NLSY.

Given the consistency of our findings, we conclude that the estimated effects of education on crime cannot be easily explained away by unobserved characteristics of criminals, unobserved state policies that affect both crime and schooling, or educational differences in the conditional probability of arrest and imprisonment given crime. Evidence from other studies regarding the elasticity of crime with respect to wage rates suggests that a significant part of the measured effect of education on crime can be attributed to the increase in wages associated with schooling.

We further argue that the impact of education on crime implies that there are benefits to education not taken into account by individuals themselves, so the *social return* to schooling is

larger than the private return. The estimated social externalities from reduced crime are sizeable. A 1% increase in the high school completion rate of all men ages 20-60 would save the United States as much as $1.4 billion per year in reduced costs from crime incurred by victims and society at large. Such externalities from education amount to $1,170-2,100 per additional high school graduate or 14-26% of the private return to schooling. It is difficult to imagine a better reason to develop policies that prevent high school drop out.

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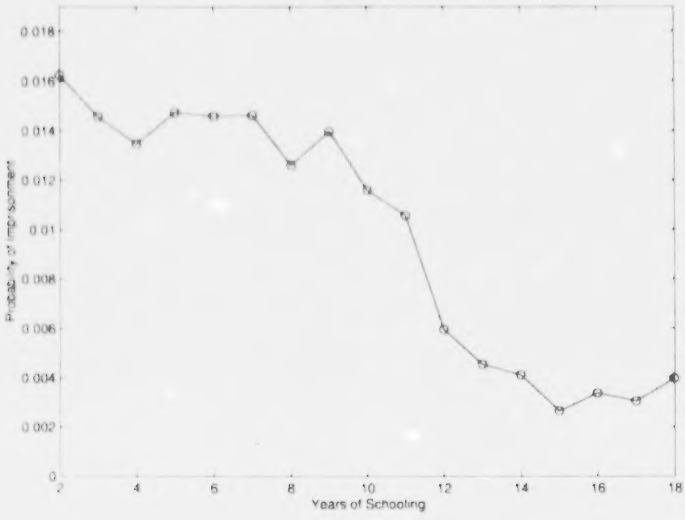
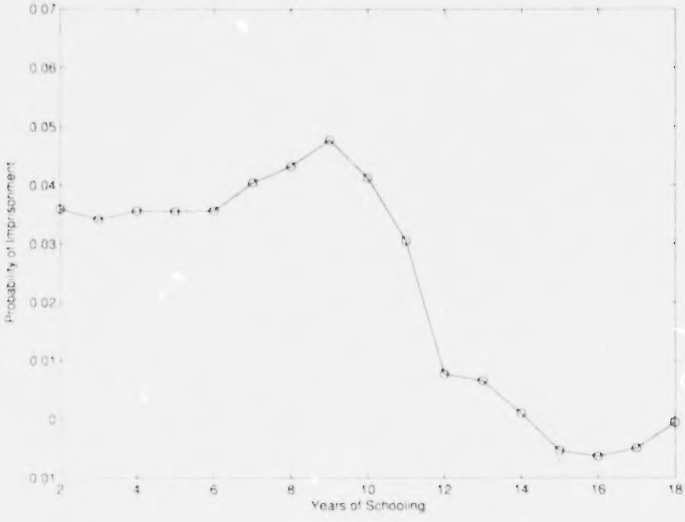
Figure 1: Regression Adjusted Probability of Incarceration, by Years of Schooling

WHITES



BLACKS



Note: Regression-adjusted probability of incarceration is obtained by conditioning on age, state of birth, state of residence, cohort of birth, and year effects.

Figure 2: Changes in Compulsory Attendance Laws by State 1914-1978

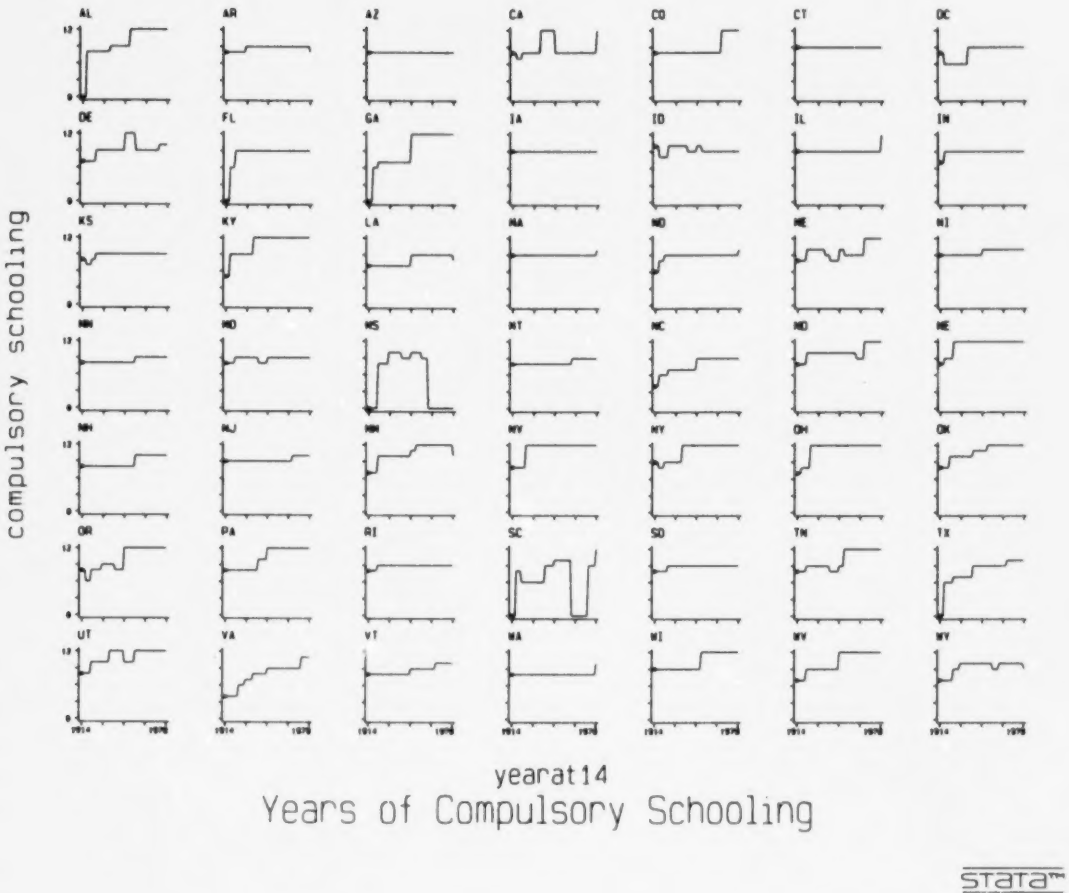


Figure 3: The Effect of Increases in Compulsory Attendance Laws on Average Years of Schooling

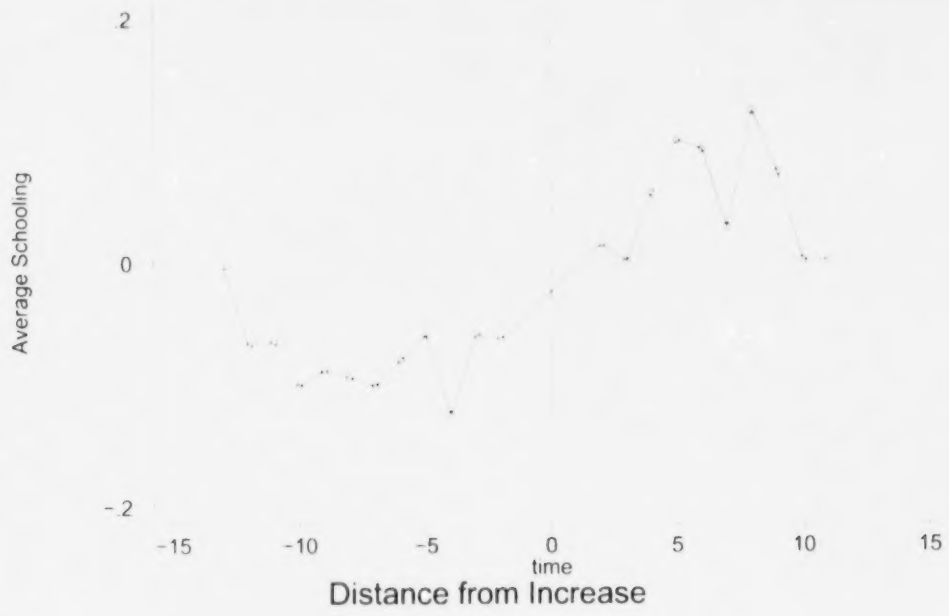


Table 1: Census Descriptive Statistics: Mean (Standard Deviation) by Year

Variable	1960	1970	1980
In Prison	0.0067	0.0051	0.0068
	(0.0815)	(0.0711)	(0.0820)
Years of Schooling	10.54	11.58	12.55
	(3.56)	(3.39)	(3.07)
High School Graduate +	0.48	0.63	0.77
	(0.50)	(0.48)	(0.42)
Age	38.79	38.54	37.00
	(11.21)	(11.95)	(11.94)
Compulsory Attendance ≤ 8	0.32	0.20	0.14
	(0.46)	(0.40)	(0.35)
Compulsory Attendance = 9	0.43	0.45	0.40
	(0.49)	(0.49)	(0.49)
Compulsory Attendance = 10	0.06	0.07	0.09
	(0.24)	(0.26)	(0.29)
Compulsory Attendance ≥ 11	0.17	0.26	0.34
	(0.37)	(0.44)	(0.47)
Black	0.096	0.090	0.106
	(0.295)	(0.287)	(0.307)
Sample Size	392,103	880,404	2,694,731

Table 2: Census Incarceration Rates for Men by Education

	All Years	1960	1970	1980
White Men				
HS Drop Out	.0083	.0076	.0069	.0093
HS Graduate	.0034	.0021	.0022	.0039
Some College	.0024	.0021	.0013	.0027
College +	.0007	.0003	.0002	.0008
Black Men				
Drop Out	.0364	.0294	.0294	.0411
HS Graduate	.0218	.0180	.0152	.0235
Some College	.0197	.0081	.0089	.0215
College +	.0066	.0000	.0026	.0075

Notes: High school drop outs are individuals with less than 12 years of schooling or 12 years but no degree; high school graduates have exactly 12 years of schooling and a high school degree. Individuals with some college have 13-15 years of schooling, and college graduates have at least 16 years of schooling and a college degree.

Table 3: OLS and IV Estimates of the Effect of Years of Schooling on Imprisonment

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
WHITES						
Second-Stage						
Years of Schooling	-.0010	-.0010	-.0010	-.0011	-.0009	-.0014
	(.00002)	(.00002)	(.00002)	(.0002)	(.0005)	(.0006)
First Stage						
Compulsory Attendance = 9				0.278	0.222	0.202
				(0.026)	(0.024)	(0.024)
Compulsory Attendance = 10				0.213	0.199	0.176
				(0.035)	(0.034)	(0.033)
Compulsory Attendance $\geq$ 11				0.422	0.340	0.329
				(0.037)	(0.033)	(0.033)
First Stage R-squared				0.12	0.13	0.13
F-test for Instruments				52.5	38.6	36.2
BLACKS						
Second-Stage						
Years of Schooling	-.0037	-.0037	-.0037	-.0047	-.0033	-.0041
	(.0001)	(.0001)	(.0001)	(.0012)	(.0018)	(.0019)
First Stage						
Compulsory Attendance = 9				0.672	0.454	0.421
				(0.043)	(0.040)	(0.039)
Compulsory Attendance = 10				0.664	0.476	0.434
				(0.079)	(0.071)	(0.070)
Compulsory Attendance $\geq$ 11				0.794	0.528	0.509
				(0.068)	(0.063)	(0.062)
First Stage R-squared				0.25	0.25	0.26
F-test for Instruments				88.1	45.9	41.5
Additional Controls:						
Cohort of Birth Effects		Y	Y		Y	Y
State of Residence $\times$ Year Effects			Y			Y

Notes: Standard errors corrected for state of birth - year of birth clustering are in parentheses. The dependent variable is a dummy equal to 1 if the respondent is in prison. All specifications control for age, year, state of birth, and state of residence. Sample in the top panel includes white males ages 20-60 in 1960, 1970, and 1980 Censuses; N = 3,209,138. Sample in the bottom panel includes black males ages 20-60 in 1960, 1970, and 1980 Censuses. N = 410,529. Age effects include 14 dummies (20-22, 23-25, etc.). State of birth effects are 49 dummies for state of birth (Alaska and Hawaii are excluded). Year effects are 3 dummies for 1960, 1970, and 1980. State of residence effects are 51 dummies for state of residence. Cohort of birth effects are dummies for decade of birth (1914-23, 1924-33, etc.). Models for blacks also include an additional state of birth dummy for cohorts born in the South turning age 14 in 1958 or later to account for the impact of Brown v. Board of Education.

Table 4: OLS and IV Estimates of the Effect of High-School Graduation on Imprisonment

	OLS		2SLS	
	(1)	(2)	(3)	(4)
WHITES				
HS Graduate	-0.0077	-0.0077	-0.0061	-0.0089
	(0.0002)	(0.0002)	(0.0035)	(0.0037)
Hausman Test [p-value]				
First Stage F-test			47.91	48.05
BLACKS				
HS Graduate	-0.0339	-0.0339	-0.0723	-0.0800
	(0.0011)	(0.0011)	(0.0366)	(0.0378)
Hausman Test [p-value]				
First Stage F-test			10.09	10.01
Additional Controls:				
State of Residence × Year Effects		y		y

Notes: Standard errors corrected for state of birth - year of birth clustering are in parentheses. The dependent variable is a dummy equal to 1 if the respondent is in prison. High school graduation is a dummy equal 1 if the respondent has 12 or more years of schooling, and a high school degree. All specifications control for age, year, state of birth, and state of residence. Sample in the top panel includes white males ages 20-60 in 1960, 1970, and 1980 Censuses; N = 3,209,138. Sample in the bottom panel includes black males ages 20-60 in 1960, 1970, and 1980 Censuses. N = 410,529. Age effects include 14 dummies (20-22, 23-25, etc.). State of birth effects are 49 dummies for state of birth (Alaska and Hawaii are excluded). Year effects are 3 dummies for 1960, 1970, and 1980. State of residence effects are 51 dummies for state of residence. Cohort of birth effects are dummies for decade of birth (1914-23, 1924-33, etc.). Models for blacks also include an additional state of birth dummy for cohorts born in the South turning age 14 in 1958 or later to account for the impact of Brown v. Board of Education.

Table 5: The Effect of Compulsory Attendance Laws on Schooling Achievement

	(1) drop out	(2) high school	(3) some college	(4) college+
WHITES				
Compulsory Attendance = 9	-.0325	.0327	-.0004	.0003
	(.0034)	(.0037)	(.0017)	(.0020)
Compulsory Attendance = 10	-.0331	.0401	-.0030	-.0039
	(.0045)	(.0051)	(.0030)	(.0033)
Compulsory Attendance $\geq$ 11	-.0551	.0582	-.0068	.0036
	(.0047)	(.0052)	(.0026)	(.0032)
F-test (p-value)	0.0000	0.0000	0.027	0.171
R-squared	0.12	0.02	0.04	0.05
BLACKS				
Compulsory Attendance = 9	-.0236	.0309	-.0069	-.0003
	(.0046)	(.0041)	(.0023)	(.0016)
Compulsory Attendance = 10	-.0176	.0406	-.0182	-.0047
	(.0065)	(.0064)	(.0039)	(.0023)
Compulsory Attendance $\geq$ 11	-.0296	.0502	-.0189	.0016
	(.0069)	(.0062)	(.0034)	(.0025)
F-test (p-value)	0.0000	0.0000	0.0000	0.136
R-squared	0.19	0.07	0.06	0.02

Notes: Standard errors corrected for State of Birth - Year of Birth clustering are in parentheses. The dependent variable in column 1 is a dummy equal to 1 if the respondent is a high school drop out. The dependent variables in columns 2-4 are dummies for high school, some college, and college, respectively. All specifications control for age, year, state of birth, state of residence, and cohort of birth. Sample in the top panel includes white males ages 20-60 in 1960, 1970, and 1980 Censuses; N = 3,209,138. Sample in the bottom panel includes black males ages 20-60 in 1960, 1970, and 1980 Censuses; N = 410,529. Age effects are 14 dummies (20-22, 23-25, etc.). State of birth effects are 49 dummies for state of birth (Alaska and Hawaii are excluded). Year effects are 3 dummies for 1960, 1970 and 1980. State of residence effects are 51 dummies for state of residence. Cohort of birth effects are dummies for decade of birth (1914-23, 1924-33, etc.). Models for blacks also include an additional state of birth dummy for cohorts born in the South turning age 14 in 1958 or later to account for the impact of Brown v. Board of Education.

Table 6: Are changes in compulsory attendance laws correlated with the number of policemen or state police expenditures?

	Number of Policemen (1)	Police Expenditures (2)	Per Capita Police Expend. (3)
Compulsory Attendance =9	.0024	.103	-.002
	(.0080)	(.186)	(.002)
Compulsory Attendance =10	-.0031	-.430	-.015
	(0.0104)	(.209)	(.003)
Compulsory Attendance =11	-.0080	.-340	-.011
	(.0102)	(.180)	(.003)
R-squared	0.81	0.89	0.85
N	343	1500	1500

Notes: Standard errors in parentheses. All specifications control for year and state effects. The dependent variable in column 1 is the percentage policemen in the state. Sample in column 1 includes observations from 49 states in years 1920, 1930, 1940, 1950, 1960, 1970, and 1980. The number of policemen in 1920-40 are taken from Census reports on occupations and the labor force for the entire U.S. population. Data from 1950-80 are from the IPUMS 1% Census samples. The dependent variable in column 2 is state police expenditures/$100 billions in constant dollars; Sample in column 2 includes observations from 49 states in all years from 1946 to 1978. The dependent variable in column 3 is state per capita police expenditures in constant dollars; sample in column 3 includes observations from 49 states in years all years from 1946 to 1978. Data on police expenditures are from ICPSR 8706. See text for details.

Table 7: The Effect of Future Compulsory Attendance Laws on Current Graduation Status

	WHITES			BLACKS		
	Compuls. Att. = 9	Compuls. Att. = 10	Compuls. Att. ≥11	Compuls. Att. = 9	Compuls. Att. = 10	Compuls. Att.≥ 11
	(1)	(2)	(3)	(4)	(5)	(6)
t = +4	-.0032	.0025	-.0141	.0054	-.0153	-.0164
	(.0122)	(.0182)	(.0214)	(.0067)	(.0110)	(.0144)
t = +5	.0004	.0085	-.0007	-.0004	-.0098	-.0068
	(.0078)	(.0113)	(.0141)	(.0046)	(.0081)	(.0101)
t = +6	.0006	.0100	.0027	-.0043	-.0132	-.0160
	(.0069)	(.0093)	(.0121)	(.0045)	(.0073)	(.0095)
t = +7	.0001	.0107	.0027	-.0072	-.0136	-.0024
	(.0057)	(.0078)	(.0121)	(.0043)	(.0079)	(.0090)
t = +8	.0013	.0106	.0091	-.0099	-.0106	-.0047
	(.0054)	(.0071)	(.0086)	(.0042)	(.0079)	(.0083)
t = +9	.0016	.0092	-.0094	-.0126	-.0104	-.0060
	(.0051)	(.0067)	(.0080)	(.0041)	(.0079)	(.0070)
t = +10	.0011	.0095	.0123	-.0140	-.0084	-.0041
	(.0046)	(.0063)	(.0071)	(.0045)	(.0078)	(.0075)
t = +11	-.0013	.0063	.0131	-.0156	-.0071	-.0020
	(.0043)	(.0055)	(.0069)	(.0049)	(.0075)	(.0078)
t = +12	-.0061	.0016	.0080	-.0158	-.0017	-.0042
	(.0047)	(.0054)	(.0072)	(.0050)	(.0070)	(.0075)
t = +15	-.0092	-.0018	.00078	-.0097	.0122	-.0044
	(.0046)	(.0054)	(.0066)	(.0052)	(.0063)	(.0079)
t = +18	-.0067	.0019	.0131	-.0020	.0271	-.0061
	(.0046)	(.0055)	(.0056)	(.0055)	(.0061)	(.0085)
t = +20	-.0065	.0040	.0076	.0013	.0349	.0040
	(.0050)	(.0060)	(.0059)	(.0064)	(.0071)	(.0083)

Notes: Standard errors corrected for State of Birth - Year of Birth clustering are in parentheses. The dependent variable is a dummy equal to 1 if the respondent is a high school graduate. Each row is a separate regression. All models control for compulsory attendance laws at t=0, t=1, t=2 and t=3, as well as year, age, state of birth, state of residence, and cohort of birth. Age effects are 14 dummies (20-22, 23-25, etc.). State of birth effects are 49 dummies for state of birth (Alaska and Hawaii are excluded). Year effects are 3 dummies for 1960, 1970 and 1980. State of residence effects are 51 dummies for state of residence. Cohort of birth effects are dummies for decade of birth (1914-23, 1924-33, etc.). Columns 4, 5, and 6 also include an additional state of birth dummy for cohorts born in the South turning age 14 in 1958 or later to account for the impact of Brown v. Board of Education. In column 1, 2 and 3, sample includes white males ages 20-60 in 1960, 1970, and 1980 Censuses. In column 4, 5, and 6 sample includes black males ages 20-60 in 1960, 1970, and 1980 Censuses. N = 3,209,138 for whites; N = 410,529 for blacks.

Table 8: The Impact of Years of Schooling on Incaceration Robustness Checks

	WHITES		BLACKS	
	OLS	2SLS	OLS	2SLS
	(1)	(3)	(3)	(4)
(A) Base Case	-.0010	-.0014	-.0037	-.0041
	(.00002)	(.0006)	(.0001)	(.0019)
First Stage F-Test		36.2		41.5
(B) Region of Birth*Cohort Trend	-.0010	-.0019	-.0037	-.0073
	(.00002)	(.0010)	(.0001)	(.0026)
First Stage F-Test		12.79		28.39
(C) Region of Birth*Cohort Effects	-.0010	-.0022	-.0037	-.0034
	(.00002)	(.0017)	(.0001)	(.0035)
First Stage F-Test		5.74		22.41
(D) State of Birth*Cohort Trend	-.0010	-.0034	-.0037	-.0067
	(.00002)	(.0021)	(.0001)	(.0032)
First Stage F-Test		5.83		19.15
(E) Age Effects*Cohort Effects	-.0010	-.0017	-.0037	-.0033
	(.00002)	(.0007)	(.0001)	(.0023)
First Stage F-Test		37.90		35.68
(F) Education	-.0015	-.0065	-.0087	-.0072
	(.0001)	(.0014)	(.0007)	(.0063)
Education * Age	.00002	.00007	.0001	.00003
	(.00000)	(.00001)	(.00001)	(.00008)
Effect at Age 20	-.0011	-.005	-.006	-.006
Effect at Age 40	-.0006	-.003	-.004	-.005
First Stage F-Test		19.2-28.8		24.3-34.8

Notes: Standard errors corrected for state of birth - year of birth clustering are in parentheses. All specifications control for age, state of birth, and state of residence × year.

Table 9: OLS and IV Estimates of the Effect of Schooling on Arrest Rates

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
(A) AVERAGE EDUCATION						
Avg. Years of Education	-0.114	-0.116	-0.111	-0.176	-0.182	-0.162
	(0.024)	(0.023)	(0.042)	(0.080)	(0.080)	(0.105)
R-squared	0.89	0.93	0.95			
(B) HIGH SCHOOL GRADUATION						
HS Graduation Rate	-0.618	-0.674	- 0.710	-0.946	-0.941	-0.873
	(0.183)	(0.181)	(0.283)	(0.491)	(0.522)	(0.669)
R-squared	0.93	0.95	0.96			
Controls:						
age × offense effects	y	y	y	y	y	y
offense × year effects	y	y	y	y	y	y
age × year effects	y	y	y	y	y	y
state × age effects	y	y	y	y	y	y
state × offense effects		y	y		y	y
state × year			y			y

Notes: Standard errors corrected for state-year-age clustering are in parentheses. The dependent variable is the logarithm of the arrest rate by age, type of offense, state, and year. Average schooling and high school graduation rate is by age group, state, and year (see text). All models control for percentage black. There are 8 age groups, 8 offenses, 50 states, and 4 years. All models are weighted by cell size.

Table 10: OLS Estimates for Arrest Rates by Type of Crime

	Average Educ.		HS Grad. Rate	
	(1)	(2)	(3)	(4)
(A) VIOLENT vs. PROPERTY CRIME				
Violent Crime	-.121	-.116	-.751	-.793
	(.025)	(.044)	(.198)	(.291)
Property Crime	-.111	-.105	-0.593	-0.621
	(.026)	(.044)	(.208)	(.304)
(B) BY DETAILED TYPE OF CRIME				
Murder	-.276	-.274	-2.062	-2.133
	(.041)	(.058)	(.403)	(.403)
Rape	.113	.118	1.094	1.049
	(.037)	(.048)	(.307)	(.353)
Robbery	-.007	-.005	0.184	0.113
	(.031)	(.047)	(.253)	(.333)
Assault	-.297	-.292	-2.136	-2.179
	(.028)	(.048)	(.226)	(.326)
Burglary	-.057	-.052	-0.202	-0.250
	(.032)	(.048)	(.268)	(.347)
Larceny	-.058	-.052	-0.235	-0.277
	(.027)	(.045)	(.209)	(.311)
Vehicle Theft	-.201	-.197	-1.227	-1.271
	(.030)	(.048)	(.251)	(.346)
Arson	-.133	-.127	- 0.745	-0.784
	(.044)	(.053)	(.358)	(.408)
Additional Controls:				
state × year		y		y

Notes: Standard errors corrected for state-year-age clustering are in parentheses. Violent crimes include murder, rape, robbery, and assault. Property crimes include burglary, larceny, vehicle theft, and arson. Average schooling and high school graduation rate are by age group, state, and year (see text). All specifications control for percentage black, age × offense effects, offense× year effects, age × year effects, state × age effects and state × offense effects. There are 8 age groups, 8 offenses, 50 states, and 4 years. All models are weighted by cell size.

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Table 11: The Effect of Education on Self-Reported Crime and Incarceration for Whites (NLSY)

	Years of School		HS Graduate	
	(1)	(2)	(3)	(4)
Self-Reported Crime				
Violent Crime	-0.0212	-0.0177	-0.1063	-0.1102
	(0.0078)	(0.0087)	(0.0218)	(0.0229)
Drug Sales	-0.0151	-0.0156	-0.0562	-0.0555
	(0.0054)	(0.0060)	(0.0153)	(0.0159)
Property Crime	-0.0184	-0.0151	-0.1155	-0.1259
	(0.0111)	(0.0122)	(0.0311)	(0.0321)
Any Crime	-0.0310	-0.0287	-0.1627	-0.1758
	(0.0122)	(0.0135)	(0.0341)	(0.0355)
Incarcerated	-0.0053	-0.0056	-0.0331	-0.0315
	(0.0005)	(0.0007)	(0.0027)	(0.0032)
Controls:				
Age/Cohort	y	y	y	y
Region of Residence	y	y	y	y
Enrolled in School	y	y	y	y
Family Background		y		y
Ability		y		y
SMSA Status		y		y
Local Unemployment Rate		y		y
State Incarceration Rate		y		y

Notes: Self-reported crimes are based on men ages 18-23 in 1980. Violent crimes correspond to robbery and assault, while property crimes include shop-lifting and all other thefts of over $50. Each row represents a separate OLS regression. The dependent variables for the self-reported crimes are dummy variables equal to one if the person participated in that type of crime; in for incarceration, it is a dummy equal 1 if the individual was incarcerated at any time over ages 22-28. The reported coefficients for incarceration are obtained by adjusting the ages 22-28 incarceration rates by the ratio of the probability of incarceration for the seven year span to the annual incarceration probability (over those same ages). Family background measures include: current enrollment in school, parents highest grade completed, whether or not the individual lived with both of his natural parents at age 14, whether his mother was a teenager at his birth, and family income.

Table 12: Social Costs per Crime and Social Benefits of Increasing High School Completion Rates by 1%

	Victim Costs per crime (1)	Property Loss per crime (2)	Incarc. Cost per crime (3)	Total Cost per crime (4)	Est. Change in Arrests (5)	Est. Change in Crimes (6)	Social Benefit (4)×(6) (7)
Violent Crimes							
Murder	2,940,000	120	845,455	3,024,359	-373	-373	$1,129,596,562
Rape	87,000	100	2,301	89,221	347	1,559	-$139,109,278
Robbery	8,000	750	1,985	9,385	134	918	-$8,617,191
Assault	9,400	26	538	9,917	-7,798	-37,135	$368,252,227
Property Crimes							
Burglary	1,400	970	363	987	-653	-9,467	$9,342,643
Larceny/Theft	370	270	44	198	-1,983	-35,105	$6,944,932
Motor Vehicle Theft	3,700	3,300	185	1,245	-1,355	-14,238	$17,728,056
Arson	37,500	15,500	1,542	39,042	-69	-469	$18,323,748
Total					11,750	94,310	$1,402,461,698

Notes: Victim costs and property losses taken from Table 2 of Miller, Cohen, and Wiersema (1996). Incarceration costs per crime equal the incarceration cost per inmate, $17,027 (Stephan 1999), multiplied by the incarceration rate (Lynch et al. 1994). Total costs are calculated as the sum of victim costs and incarceration costs less 80% of the property loss (already included in victim costs) for all crimes except arson. Total costs for arson are the sum of victim costs and incarceration costs. See text for details. Estimated change in arrests calculated from panel B, column 4 of Table 10 and the total number of arrests in 1990 Uniform Crime Reports. Estimated changes in crimes adjusts the arrest effect by the number of crimes per arrest. The social benefit is the estimated change in crimes in column 6 times the total cost per crime in column 4. All dollar figures are in 1993 dollars. See text for details.

Appendix A Interpretation of IV and OLS Estimators

When estimating the impact of high school graduation on the probability of incarceration using only an indicator variable for graduation status, OLS and IV estimators can be written as weighted sums of causal responses to each unit change in schooling (i.e. completing grades 9, 10, 11, 12, etc.), where the sets of "weights" differ for the two estimators. The results presented here build on Angrist and Imbens (1995) to show that the "weights" placed on the effects of each schooling progression have an intuitive interpretation and are functions of observable quantities. The expected difference between the estimators depends on the difference in the weights as well as the impacts of schooling on incarceration at each grade level. In our context, we empirically show that differences in these "weights" explain much of the difference between our OLS and IV estimates for blacks.

Consider the following model for incarceration: $y_i(s) = \sum_{j=0}^{S} \beta_j + \varepsilon_i$, which depends on the level of schooling $s \in \{1, 2, ..., S\}$ and a mean zero individual-specific iid error term ε_i (all individuals are assumed to have the same β's).[37] Let D be an indicator equal to one when $s \geq k$ and zero otherwise. In our case, $k = 12$, so D represents high school graduation. For purposes of this analysis, assume that D is independent of ε. The OLS regression

$$y_i = \alpha + \beta D_i + \varepsilon_i \tag{3}$$

yields an estimate for β with

$$plim\ \beta_{OLS} = E(y|s \geq k) - E(y|s < k) = \beta_k + \sum_{j=1}^{k-1} \phi_j \beta_j + \sum_{j=k+1}^{S} \theta_j \beta_j \tag{4}$$

where $\phi_j = Pr(s < j|s < k)$ and $\theta_j = Pr(s \geq j|s \geq k)$. OLS estimates of the β_j's are pictured in Figure 1.

Consider an instrument $Z \in \{0, 1, 2, ..., I\}$ that satisfies the monotonicity and independence assumptions of Angrist and Imbens (1995).[38] The two stage least squares (2SLS) estimate for β using I indicator variables ($z_i = 1$ if $Z = i$ and zero otherwise) as instruments for D in equation (3) has

$$plim\ \beta_{2SLS} = \frac{E\{y|E(D|Z) - E(D))\}}{E\{E(D|Z)[E(D|Z) - E(D))\}} = \beta_k + \sum_{j \neq k} \lambda_j \beta_j, \tag{5}$$

where

$$\lambda_j = \frac{\sum_{i=1}^{I} \Pr(Z = i)[E(D|Z = i) - E(D)]Pr(s_i \geq j > s_0)}{\sum_{i=1}^{I} \Pr(Z = i)[E(D|Z = i) - E(D)]Pr(s_i \geq k > s_0)}. \tag{6}$$

[37] For a discussion of IV estimation and random coefficient models, see Heckman (1997), Heckman and Vytlacil (2001), and Imbens and Angrist (1994).

[38] In our case, $Z = 0$ if compulsory schooling is 8 years or less; $Z = 1$ if compulsory schooling is 9 years; $Z = 2$ if compulsory schooling is 10 years; $Z = 3$ if compulsory schooling is 11 or 12 years.

and s_z is the schooling choice for someone when $Z = z$. The λ-weights depend on the number of individuals whose education level is affected by each of the instruments ($Pr(s_i \geq j > s_0)$) and the distance between $E(D|Z = i)$ and E(D).[39] Equations (4) and (5) show that the OLS and 2SLS estimators are different weighted sums of the causal responses to each unit change in schooling. Although we refer to the ϕ's, θ's, and λ's as "weights" for lack of a better word, neither the OLS nor the 2SLS "weights" sum to 1. For both OLS and 2SLS estimators, the weights on β_{12} alone must equal one, so their sums will generally be greater than one.

In general, the OLS and 2SLS estimates need not coincide even if OLS estimates are not plagued by endogeneity problems. This is because both estimators estimate different functions of the underlying β's. Comparing the 2SLS and OLS estimates produces the following relationship:

$$plim\ (\hat{\beta}_{2SLS} - \hat{\beta}_{OLS}) = \sum_{j=1}^{k-1}(\lambda_j - \phi_j)\beta_j + \sum_{j=k+1}^{S}(\lambda_j - \theta_j)\beta_j. \qquad (7)$$

Equation (7) makes clear that only special cases will yield OLS and 2SLS estimates with the same expectation.[40] The empirical values for these weights are shown in Figure A-1, separately for blacks and whites. While the difference in 2SLS and OLS weights is small for whites, it is sizeable for blacks.

Recall that the OLS estimates for blacks are smaller than the 2SLS estimates despite the fact that endogeneity would lead us to expect the opposite. Both OLS and 2SLS estimates are similar for whites. Do the weights explain these findings? Re-weighting the OLS estimates of each causal response (as shown in Figure 1) by the 2SLS weights in Figure A-1 produces an adjusted impact of 0.0077 for whites and 0.05 for blacks. While re-weighting the OLS estimator produces little change for whites (when compared with the OLS estimates in Table 4), it substantially increases the estimated effect for blacks. This is consistent with what we observe for the 2SLS estimates in the table. Although this evidence is only suggestive, the larger 2SLS estimates for blacks seem to reflect, at least in part, the differences in weights that OLS and 2SLS give to the impacts of different schooling levels on crime. Some of the 2SLS-OLS difference may also be attributed to their imprecision. Using a standard Hausman test, we cannot reject that the OLS and 2SLS estimates are the same for both whites and blacks.

We now turn to the model that is linear in years of schooling. In this case, the 2SLS estimator of the coefficient on years of schooling equals $\sum_{j=1}^{S}\lambda_j\beta_j$, and the OLS estimator equals $\sum_{j=1}^{S}\omega_j\beta_j$.

[39] See Appendix A of Lochner and Moretti (2001) for a derivation of these λ-weights.

[40] There are two such special cases. First, schooling may have no effect on incarceration at any level except moving from 11 to 12 years. In this case, $\beta_j = 0$ for all $j \neq 12$, and the two estimators consistently estimate the effect of moving from 11 to 12 years of school. Second, $\lambda_j = \phi_j$ for all $j < 12$ and $\lambda_j = \theta_j$ for all $j > 12$. In this unlikely event, both estimators will yield the same weighted sums of all causal effects.

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The 2SLS weights are

$$\lambda_j = \frac{\sum_{i=1}^{I} \Pr(Z=i)[E(s|Z=i) - E(s)]Pr(s_i \geq j > s_0)}{\sum_{i=1}^{I} \Pr(Z=i)[E(s|Z=i) - E(s)][E(s|Z=i) - E(s|Z=0)]}.$$

The OLS weights are $\omega_j = [Pr(s \geq j)(E(s|s \geq j) - E(s))]/Var(s)$. For a detailed discussion of OLS weights in a continuous regressor context, see Yitzhaki (1996). Unlike the previous weights, these 2SLS and OLS weights must sum to one: $\sum_{j=1}^{S} \hat{\lambda}_j = \sum_{j=1}^{S} \omega_j = 1$. They are weighted averages of all causal impacts, where the weights are shown in Figure A-2. The differences between OLS and 2SLS weights are small for both blacks and whites, consistent with the small difference in point estimates based on the linear specification shown in Table 3.

Finally, we turn to the effect of moving from 11th grade to 12th grade. If the relationship between education and incarceration is non-linear as suggested by Figure 1, what do our 2SLS estimates say about the impact of completing high school (i.e. moving from 11th grade to 12th grade)? As discussed above, the 2SLS estimator using an indicator for high school graduation will over-estimate this effect. On the other hand, if the effect of completing 12th grade is greater than the impact of any other single-year grade transition, the linear-in-schooling 2SLS estimator will under-estimate the effect of finishing the 12th grade since it is a weighted average of all single-year causal effects. Thus, the effect of finishing 12th grade should be bounded by the linear-in-schooling and the graduate dummy specifications.

Figure A-1: 2SLS and OLS Weights for the Model that Includes a Dummy for HS Graduate

Figure A-2: 2SLS and OLS Weights for the Model Linear in Years of Schooling

The lines with circles plot 2SLS weights for each year of schooling; the lines with triangles plot OLS weights. The lines with '+' plot the difference between 2SLS and OLS weights.

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